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All communications should be addressed to the Editor.

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Notices of the Aëronautical Society of Great Britain.

New Members.

The following have been elected members of the Society in the various grades:—

Fellow.

Harris Booth, B.A., A.M.Inst.C.E., F.R.Met.Soc., A.F.Aë.S.

Associate Fellows.

Alan Ernest Leofric Chorlton, C.B.E., M.Inst.C.E., M.Inst.Mech.E., M.Inst.E.E., M.Sc., Allan Lindsay Anderson, Granville Eastwood Bradshaw, Alfred John Sutton Pippard, M.Sc., A.M.Inst.C.E., Capt. Hon. W. F. Forbes Sempill, R.F.C., Harold Lord, William Herbert Hatfield, D.Sc., Edgar Alfred Allcutt, A.M.I.C.E., M.Sc., A.M.I.M.E., Lieut. Samuel Turner, Ivan Owen Williams, B.Sc., A.M.I.C.E., Henry Edgar Broadsmith.

Members.

G. D. I. Seneviratne, Lieut.-Col. R. Mandeville Rodwell, R.F.C., Comdr. William Bernard Murphy, R.N., Arthur Francis Martin, Howard Edward Latimer Wright, William Elderton, Capt. Frank Bernard Harford, R.F.C., Albert Edward Banett, Capt. Carleton George Chapman, Aviation Service, U.S.A. Army, Major J. Baird, M.P.

Associate Members.

Lieut. B. Hackforth, R.N.A.S., Major Leveson Scarth, William R. Catesby, William Stevenson Storie, Arthur W. Cutbill, C. H. Wallis.

Students.

David Maxwell Buist, Charles Watson Hall, Laurence Dove, Edwin H. Moyes, George Isaac Price, Eric A. Seal.

Obituary.

The Council record with great regret the death of Lieutenant-Colonel Clive Maitland Waterlow, R.E., who was killed on July 17th, as the result of an accident while flying. Colonel Waterlow was one of the most valued members of the Society and had been an Associate Fellow since 1911. He was attached to the R.N.A.S.

Civil Aerial Transport Committee.

In the last issue of this journal it was inadvertently stated that General Ruck had been invited to act on the above committee as representative of this Society. This statement was incorrect, for the committee does not consist of representatives but of members selected for their individual qualifications.

Honours and Awards.

The following members of the Society have been appointed to the Most Excellent Order of the British Empire for services in connection with the war:—

To be Knight Grand Cross.

The Right Hon. Lord Sydenham, G.C.S.I., G.C.M.G., G.C.I.E., F.R.S.

To be Knight Commander.

Sir Dugald Clerk, F.R.S., F.Aë.S., M.Inst.C.E.

To be Commander.

Leonard Bairstow, F.R.S., A.F.Aë.S., Alan Ernest L. Chorlton, M.Inst.C.E., M.Inst.E.E., M.Inst.M.E., M.Sc., A.F.Aë.S.

Col. J. Dewar Cormack, C.M.G., A.F.Aë.S., has been appointed Brigadier-General.

RECENT ADDITIONS TO THE LIBRARY.

"Canadian Patent Office Record." Vol. XLV. Ottawa. 1917.

"Edward Teshmaker Busk." By M.B. 1917. Glasgow: Maclehose and Co.

"World's Air Routes." Lord Montague of Beaulieu. 1917.

"Aeroplane Design." F. S. Barnwell and W. H. Sayers. 1917. London. 2s. 6d. McBride, Nash and Co.

"All the World's Aircraft." C. G. Grey. 1917. London: Sampson Low. 21s.

"Theory of a Streamline." Sir Geo. Greenhill. London. 1910. Darling and Son. 5s.

STEELS USED IN AERO WORK.

BY DR. W. H. HATFIELD (BROWN-FIRTH RESEARCH LABORATORY, SHEFFIELD).

Introduction.

Before proceeding, I wish to say that I much appreciate the invitation to present this paper to your Society. The science of aeronautics has evolved at a great pace, and typifies what can be done with applied science, and the tangible results are so apparent that it would be superfluous to discuss them. It would appear to me that the rapidity with which the efficiency has increased is due to the whole-hearted collaboration of the different branches of applied science and engineering, the honours being shared after giving first place to the sporting instinct of the early flyer, by the physicist, the engineer, and the metallurgist. You will notice that my modesty places the metallurgist last. It is quite a confession to make, but it is a fact, nevertheless, that I accepted with pleasure your invitation to give this paper, principally because I have always taken a keen interest in the metallurgical side of aeronautics. It has been a pleasure to me during the war to be in intimate contact with many of the representatives of the Air Services and with the engineers and metallurgists who have been, and are, actively working in this field, all obviously working with a keen desire to improve their technique from all standpoints. I hope this evening to discuss, and probably to raise, several points which I know to be of immediate interest, and at the same time trust to give some reliable data which I hope will be of service. As Metallurgical Expert to large producers of aero steels, I naturally fully appreciate that there are many points concerning the steels and their applications which merit very full discussion. To begin with, there is no direction in which it is so essential that a complete understanding of the properties of the material employed should be obtained, or where it is more desirable that a quantitative knowledge and appreciation should be acquired of the work which the different parts are called upon to perform.

My lecture deals with the uses of steel in aeronautics, and I propose to commence by indicating two essentials:—

(1) The use of high-class material.
(2) Scientific methods in works practice.

High quality not only influences the composition of the final steel, but also directly influences the method of production. The best materials are usually the most expensive, and hence the more expensive the material the greater will be the care in practice in the production of the steel of which it forms the base. To begin with, it is undesirable in special alloy steels that the sulphur and phosphorus should exceed relatively low limits; particularly does this apply to sulphur, since any sulphur which is present is present not really *in* the steel, but as sulphide of manganese " mixed " with it as a separate non-metallic inclusion readily observed by the microscope, and these non-metallic inclusions, *i.e.*, sulphides, slag, etc., have quite a serious influence under particular circumstances.

With regard to the necessity for scientific methods in works practice, I think that I shall have the support of everyone in my contention. If engineers

responsible for the building of aeroplanes, aero engines, etc., are to obtain anything like the best results from the materials which are now available, it is necessary that they shall have some appreciation of the characteristics and properties of those steels. They should understand the reason for the various heat treatments, and should also appreciate the considerations which lead to the selection of any particular one as the means of placing the steel in the final condition in which it may be expected to do the best service. It will be apparent that accurate heat treatment, based upon the definite thermal phenomena associated with each particular steel, is essential, and this necessitates, in works practice, the use of satisfactory and properly-standardised pyrometers. Having obtained satisfactory pyrometers, it is necessary to learn to use them intelligently, and I will here point out that it is not enough to know that your pyrometer gives a correct reading when placed into a certain temperature. What is still more important is that the temperature recorded truly represents the heat of the part which is being heat-treated. That brings us to a consideration of the types of furnaces which may be employed, and here again it is necessary to use the very best types and the most efficiently-constructed and worked furnaces if homogeneous results are to be anticipated in the final product. Later in my paper I think it will be obvious that there are a number of very essential points which will arise quite sufficient to make it totally unnecessary for me to say any more with regard to the necessity of scientific methods. It will be quite clear that the engineer responsible for producing aeroplane parts should have a sound appreciation of the metallurgical side of the business.

Before proceeding with a discussion of the constitution and properties of the steels employed in aero work, I think it desirable that I should make a few comments upon the duties the parts have to perform, factors of safety, and lastly, on the means employed in mechanically testing the steel. The last item seems to me to be extremely important, since every mechanical test is in the nature of a test to destruction, and if properly studied will give invaluable data with regard to the properties and behaviour of the material under stresses, normal and abnormal, which are likely to be encountered in practice.

Duties of Various Parts.

In deciding the class of material suitable for the various parts of any machine or structure, it is necessary to take very carefully into consideration the nature of the stresses which the parts have to withstand and the conditions under which they are called upon to work. Generally speaking, it is possible to divide them into two broad classes :—

(1) Parts subjected to stresses of a known nature and of fairly determinable magnitude.

These include steady, intermittent, and alternating stresses, and the parts concerned are those which have to take up the weight of the structure or to transmit forces directly consequent upon the generation and utilisation of power.

(2) Parts subjected to sudden stresses of unknown magnitude, generally described as " shocks."

In applying this method of division to aeroplanes, it would appear that, whilst the wings and framework of the machine as a whole may be subjected to stresses of the second class, such stresses can be, and are, to a large extent eliminated from the power plant. The aero engine, whether on the ground or in the air, is not liable to such sudden changes of load as a motor-car engine (owing to the fact that the engine and propeller can run at speeds not in a direct ratio to the forward velocity of the aeroplane), and, incidentally, there is the absence of change-gears. Comparatively light " shocks " are suffered by the valves, and their driving mechanism (cams, cam shafts, and tappets), and also by the valve seating.

The wheels and landing chassis and, in a lesser degree, the fuselage and wings, are subject to shocks, although for the latter the spring suspensions transmit the forces more as live loads than as real shocks.

Returning to the parts subjected to the first class of stress, the demand made upon these is very high. Not only do the stresses fluctuate considerably, but some of the parts, such as cylinder and piston, have at the same time to withstand high temperatures as well as abrasion. All parts also have to encounter low temperatures, and as to exactly what low temperatures have to be encountered when the planes are up at high altitude in cold weather, I should like to receive information during the discussion.

It is generally accepted that parts not subjected to shock should be produced with material having essentially a high elastic limit with just sufficient ductility to ensure non-brittleness, whereas for parts subjected to severe shock toughness is the more important feature. The division between the two classes of stresses is not very sharp, and in many cases a compromise would appear to be made between the two types of material. Bearing on this, it should always be remembered that all the parts under consideration are of relatively small section and lend themselves to very careful heat treatment, and the steels are worth studying with a view to determining exactly what properties can conveniently be introduced into such sections.

In general design there is another quality which must not be lost sight of, *i.e.*, the rigidity of the parts concerned. Parts which are not subjected to very great forces, such as the crank case in a good design, must not have their dimensions so reduced as to make them very liable to change their shape. A high-tensile material in such a case is unnecessary, it being preferable to use a light alloy and increased dimensions. (Modulus of elasticity in steel varies very little indeed either by hardening, tempering, or modification of composition within the limits of those steels used.)

The " modulus of elasticity " of the material employed is a determining factor in the question of rigidity. A lower value of the " modulus " means a greater elastic deformation under a given applied force, *i.e.*, a reduction in " stiffness," unless the dimensions are also modified. Considering, for example, a flat portion of the shell of the crank case having uniform thickness, the " stiffness " of this part to resist forces applied at right angles to it is proportional to the cube of the thickness, while its actual strength is only proportional to the square of the thickness. These laws will apply approximately to the strength and stiffness of the whole crank case.

Thus, suppose a steel casing is replaced by one of aluminium alloy of twice the thickness, and suppose the safe stress of the latter in tension is one-third of that of the steel; then taking the values of specific gravity as 7.8 and 2.8 respectively and those of Young's modulus as 14,000 and 4,700 tons per square inch respectively, a flat portion of the aluminium alloy casing would have .718 of the weight, 1.33 times the strength, and 2.66 times the stiffness of those of the corresponding part of the steel casing.

A judicious insertion of " stiffening " webs on such a casing greatly adds to its rigidity.

Comments upon Factors of Strength.*

It has been pointed out on many occasions that the use of the term " factor of safety " is misleading, and yet the erroneous use of this term still continues. It would appear to me that a true " factor of safety " should be the ratio between the stress which may be safely applied indefinitely under the actual working

* The factor of strength as fixed by the authorities is well known to be so many times the load borne by the supporting surfaces during straight horizontal flight.

conditions to the stress actually employed, and not the ratio of ultimate stress of the material under a static test to stress thought to exist in practice, as calculated from imperfect data combined with many doubtful assumptions. This latter ratio, whilst very useful in formulating empirical rules, should really come under a very different name, such as, for instance, " factor of contingency," already proposed by Mr. Lanchester.

The actual values of this factor, employed in modern design, have been arrived at largely by the method of trial. I would suggest that better results might be obtained, and fewer mysterious failures result, if the various contingencies to be allowed for are carefully examined and the " factor " placed on a more definite basis. A few of these contingencies may be mentioned. In the first place, I might mention low elastic limit as compared with maximum stress. The difficulty of measuring the true elastic limit militates against this fundamentally important figure being determined as frequently as it should be, and whilst mentioning this I would say that our ignorance as regards the properties of steels below the yield-point is, in my opinion, considerable, whilst of the properties between the yield and the breaking stress it is still greater. The Wohler phenomena also merit the most careful consideration. The rapidity of load is also extremely important. Variation of temperature, too, has a recognisedly marked influence upon the physical properties of steel, whether reference is made to high temperatures or to low temperatures, and influence of the ranges of temperature to which the parts have to be submitted in practice should be carefully studied as regards the effect upon the properties of the several steels under consideration and more use made of metallurgical research in this direction. Unequal distribution of stress, particularly near the shoulders, is a well-worn theme. It cannot be too often insisted that sharp corners, or shoulders with fillets of small radius, are bad design. Their evil effects are more marked under repeated stresses than under steady stress. The strength of a member may be reduced 50% or more due to this cause.* The reason is that local deformation may occur which is deadly when alternating stresses have to be incurred. Corrosion might also be carefully and profitably considered, particularly as regards seaplanes, where salt water has to be taken into account. Other items which appear to me to require more attention than they do receive are :—

(1) The internal stresses left after forging and probably also after heat treatment.

(2) Non-uniformity of material. This particularly applies to non-uniform conditions produced in the materials by unskilful methods of manufacture and heat treatment.

(3) Non-reliability of material is extremely important, as I shall no doubt illustrate to you, and it should be borne in mind that here the methods of production and the care which should be taken by the steel-maker are the important factors ; inclusions of slag, exaggerated sulphide inclusions, defects caused by unsatisfactory treatment in the forge, and last, but not least, hardening cracks.

These items are extremely important, and admittedly, with possibly the exception of the last-mentioned one, outside the control of the engineer, but at the same time they should be taken very seriously into account, more from the standpoint of insisting that the materials and treatments are of such a standard as he has a right to expect for such important work as that under discussion.

Whilst discussing this question of " factor of safety " may I suggest that the several parts with which we have to deal in aero work are really sufficiently small to permit conveniently of numerous quantitative destruction tests, and if more of these were made the engineer would be better able, in a practical manner,

*Stanton & Bairstow, Proc. Inst. C.E., 1906, Vol. CIXVI.

to obtain definite data upon which further progress in design might reasonably be expected. After all, a designer endeavours to do that which is almost humanly impossible, viz., to take into consideration all the factors which enter into his particular product, whereas destruction tests do that automatically if properly designed.

Parts in Detail.

In cases of failure it not infrequently happens that the metallurgist is called upon to state the cause of trouble. Now the first thing he must do is to form a

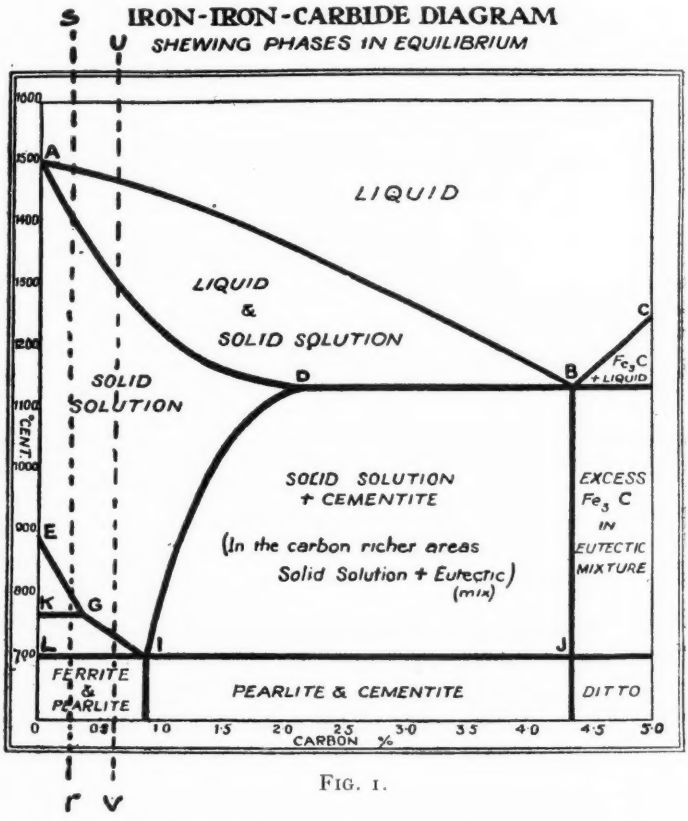
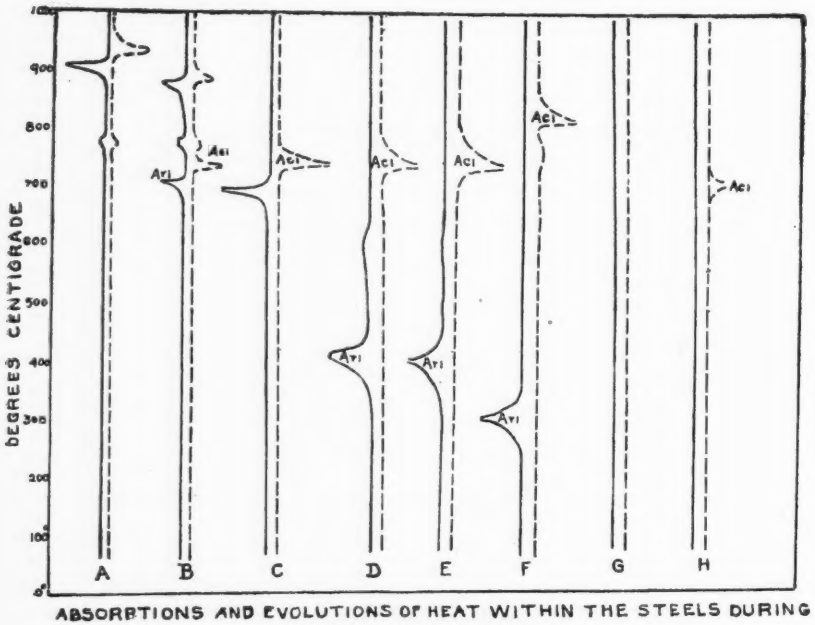


FIG. 1.

mental picture of the stresses in operation when the part was doing its work. This is rather an exacting demand, and I think it would be extremely useful if you engineers would endeavour to work out for yourselves, and incidentally for the benefit of the metallurgist, definite quantitative maximum stress combinations to which your parts of varying design may in service be subjected. I now propose to qualitatively discuss one or two of the items, and hope the discussion will amend and extend any observations which are made.

The crankshaft is one of the most important parts, and requires very careful study. No doubt engineers look with pride upon efficient designs, and yet even such fail at times. Now, if failure takes place, over-stressing has been a fact,

whatever may be the ingenious argument of the designer, and hence—and this is a typical case—not only must one examine the design and material of the crank-shaft, but also, as my friend Mr. Dickinson* has pointed out, the influence of the various other items with which it is in operation. It is desirable that an engine should develop a steady torque, and that it should run as free from



ABSORBTIONS AND EVOLUTIONS OF HEAT WITHIN THE STEELS DURING HEATING AND COOLING.

FIG. 2.

Size of Recalescence Piece = 1in. by ½in. dia.
---- Heating Curves.
——— Cooling Curves.
A Pure iron.
B Case-hardening steel.
C Carbon steel.
D High tensile steel.
E Air-hardening steel.
F Stainless steel.
G 25% nickel steel.
H Manganese steel.

vibration as possible. This, we understand, is usually accomplished by a suitable arrangement of the cylinders and cranks, such that (1) the torque developed by one cylinder shall level up the irregularities in point of time in the development of torque by the others; (2) the inertia forces consequent on the high rate of revolution shall be so disposed as to give dynamic balance. These are two distinct requirements, and the satisfaction of one does not necessarily mean the satisfaction of the other. Modern design, however, appears to approach very nearly to ideal theoretical conditions in these respects. Attention must also be

* Institution of Automobile Engineers, November, 1915.

given to the fluctuation of torque, and to the effects of inertia forces in the individual parts of the engine. In a multiple-throw crankshaft the variation of torque at the point near the portion just before the last crank may be from the maximum to zero, or to a negative value. If the shaft is of uniform strength, failure is to be expected at such a point rather than at the point where the stress is more uniform. Bending stresses due to overhanging propeller, etc., would, of course, modify this, and bending at intermediate points due to pressure on crank pin must also be considered. It is stated† that trouble frequently arises from insufficient stiffness. A certain amount of flexibility of the shaft under torque should be rather an advantage than otherwise, but it is essential that the crank case and bearings should be rigidly fixed so as to prevent undue bending action coming on the crankshaft. Accurate fitting is essential to provide this, and at the same time permit free rotation of the shaft in the bearings.

Special care must be taken with the design of the connecting rod. The maximum stress it has to stand is practically that due to the direct pressure from the explosion. When this takes place the crank is nearly on dead centre, and the force is received as compression. Before the engine has got up speed the force in the connecting rod is practically all compression, fluctuating from zero to maximum. At full speed the inertia forces reduce the compression in the connecting rod at explosion and set up tension at other parts of the working cycle. The full range of variation of stress has to be considered, taking into account also the bending stresses due to centrifugal action and the possible modification of stresses due to back-firing. The H section appears to be superior to the hollow tubular section in preventing buckling. Whichever shape is used, special care must be paid to the design of parts where the intermediate length is joined on to the ends which form the bearings. The transition must be very gradual. Any abrupt change will cause concentration of stresses on local areas, which may lead to trouble.

There seems to be an astonishing divergence of opinion as regards the material best suitable for pistons, cast iron, steel, *aluminium alloys* all being in use at the present time. Cast iron appears to be superior in wearing qualities, and is found less liable to seize, though this may in part be due to the greater rigidity obtained by employing greater thicknesses of metal, although at high temperatures cast iron is apt to distort and give out. One of the most important points is that the material of which the piston is made shall not deteriorate under the temperatures developed. I have on at least one occasion had definite indication that the piston-head had spent a considerable time in service above the carbon change-point, *i.e.*, above 730° C. Generally speaking, the fluctuations of temperature during a cycle of operations takes place too rapidly for such changes to materially affect the temperature of piston or cylinder walls, except just at the skin of the material. The possibility of attainment of extremely high local temperatures at the skin depends largely on the "thermal conductivity" and "specific heat" of the material, high values of these constants tending to reduce the fluctuation in temperature.

When we come to consider cylinders here again rigidity of form is an important consideration, as distortion due to temperature may put unexpected strain upon the parts. Incidentally, different materials are unequally affected in their mechanical properties with rising temperatures.

One would like to discuss and see discussed the whole of the items with which we are concerned, but it is proposed to now leave this side of the subject and pass to the subject of mechanical testing.

Mechanical Testing.

TENSILE.—The most directly useful and most used mechanical test is the

† Burls, p. 137, "Aero Engines.

tensile. The quantities usually measured are the yield-point, maximum stress, elongation per cent., and reduction of area per cent.

For many steels the yield-point is *not* a definite value, and the value put down for this item in a particular test depends on the personal factor and ideas of the operator. The method of determining the load which causes a sudden elongation, measurable with dividers, answers very well for a mild steel, but leaves a great deal to chance with the high-tensile materials—particularly when it is remembered that a pull of 65 tons per square inch on a 2in. test-piece will give 1/100in. elastic elongation. In how many works tests does the operator have the load run back from about the yield-point position to try if there has been permanent set? If yield-point is to be taken as a guide as to the value of "true elastic limit," and if elastic limit is to be taken as the basis of design, then this quantity must have much more attention than it usually receives.

The "maximum stress," as generally understood, *i.e.*, $\frac{\text{maximum load,}}{\text{original sectional area,}}$ can as a rule be determined exactly. What the maximum stress signifies may not always be the maximum stress of which the material is capable. It has been shown that it is possible with a ductile material,* by a simple modification of the shape of the test-piece, to make it withstand a higher stress than the maximum as ordinarily determined. It would be an interesting addition to the data usually obtained from the tensile test if the actual breaking load per square inch of reduced cross-section could be more frequently determined. There would be no difficulty in doing this with machines in which the adjustment of the jockey weight forward and backward on the beam is operated automatically.

The elongation and reduction of area per cent., which are usually taken as measures of "ductility," do not always increase or decrease together, and therefore do not indicate the same property of the material. In a test-piece which has suffered marked local contraction the possibilities of the material are indicated by that portion which has been most severely strained, *i.e.*, near the fracture. Since the density of the material is almost unaltered by strain the ratio of increased length to original length of the material very close to the fracture is practically the same as that of original area to contracted area. Hence percentage reduction of area is a better guide to ductility than elongation, and has the further advantage of being less dependent on the length of the test-piece.

It would be interesting to have that property of the material discussed which is operating if there is considerable elongation taken up uniformly throughout the test length, *i.e.*, without local contraction. The phenomenon may be due to the "stiffening" of the material under strain. Thus, if there should be a slight accidental local extension accompanied by contraction, the increase of strength resulting from the strain probably makes these portions stronger than the rest of the test-piece, in spite of the reduction of area. The straining which follows, therefore, takes place on the less strained parts. It may be that a careful study of the relationship between elongation and reduction might throw some light upon the relative manner in which steels will respond to stresses above their elastic limit. With materials which form a "waist," the "stiffening" under strain may still take place, but not sufficiently to compensate for the reduction of area.

In comparing different materials, it is only safe to take values from tests on bars of similar dimensions. The shape and size of test-piece affect in some measure all the quantities involved. Particularly is it almost hopeless to attempt to obtain measurements of elongation and contraction of area from tensile tests on a strip of steel to be comparable with tests from bars; in the first place, because of the variation in sectional area with the thickness of the sheet, and in the second place, because the test-piece is not properly machined and the results

* P. Breuil.

are materially affected by inequalities of any kind occurring on the exteriors of such pieces.

BENDING TEST.—The simple bend test is not appreciated, and is imperfectly understood. It may perhaps be useful to discuss it. If a square bar is bent to a very sharp radius, elastic deformation ceases after a fraction of a degree of deflection. During the plastic deformation there is considerable change of shape of the cross-section of the bar at the plane of severest bending. The outside of the bend, which is elongated, naturally suffers reduction of area, both in width and depth. The inside of the bend, which is compressed longitudinally, receives a "bulging." The diagram shows the shape assumed by a 1in. square bar of mild steel when bent to 180°, the smallest radius of the inside of the bend being approximately ¼in. The local elongation of the outside of the bend approaches 100% (reduction of area 50%), decrease of width 29%. The local compression at the inside of the bend is about 25%, with an increase of width of 42%. The surfaces of the bars are no longer plane, the transverse bulging on the inside of the bend being very marked. As bending proceeds, the position of plane of

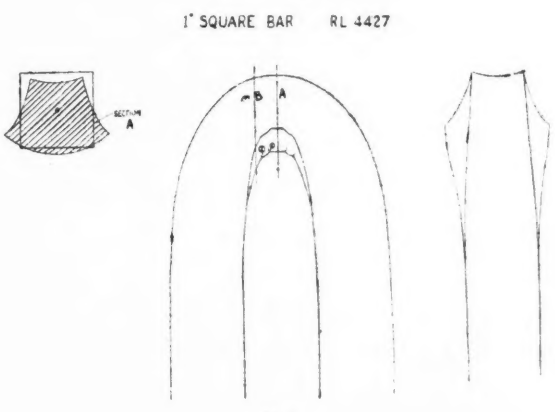


FIG. 3.

"neutral stress" is displaced towards the inside of the bend. The material which formed the original axis of the bar is, at a later stage, well inside the tension portion of the section.* The angle to which a plain bar can be bent without fracture appears to depend on the ductility of the material. Thus, if the above-mentioned bar had only been capable of, say, 25% reduction of area, fracture would have commenced at about 90° of bend. Providing very high tensile material (100 tons) can develop a sufficiently good reduction of area, other things being equal it will bend to 180°.

In Fig. 3 a bending test is illustrated drawn from an actual experiment. On the compression side of the bend several "kinks" will be noted, and the amount of deformation in structure is indicated in Micro. 2. Micro. 1 shows the general arrangement of structure before deformation. (The analysis of the material used in the experiment was: Carbon, 0.30%; manganese, 0.58%.) This little test will indicate the intensity of local deformation which can be developed, and it only needs an alternating action to produce early rupture.

IMPACT TEST.—Impact tests of the Izod and Charpy type were instituted with the object of detecting and measuring brittleness. The relative merits of

* A Martens, "Materialienkunde fur den Maschinenbau."

materials in this respect are supposed to be indicated by the amount of energy absorbed in fracturing notched bars of particular form and dimensions. In the two machines mentioned the fracture is completed by a single blow from a swinging tup. These tests have the advantages that they are very simple in operation, and give easily observed values. The justification for their use, however, must lie in the amount of success which they have had in detecting brittleness of a dangerous nature, which was not already known or could not have been easily found from other tests, and of the use which they have been in preventing the employment of unsafe materials. In this respect the genuine usefulness of the tests remains to be demonstrated.

The tests are very sensitive. They give widely-varying results for different materials. Occasionally they give widely-varying results for the same material, and, in fact, for the same test-bar, the fractured sections being less than a couple of inches apart. The variability in the results for the same material, for no *apparent* cause, forms, in my opinion, a serious objection to the test. The possible causes of variation, however, are many. An important cause is inaccuracy in preparation of test-pieces. Extreme care is required to obtain exact dimensions, as slight errors give magnified differences in readings. Even with perfect test-pieces variation is, in my opinion, an unavoidable feature of the test owing to the incidental circumstance as to how the microstructure is situated relative to the bottom of the notch. In the Izod test the condition of the material close to the bottom of the notch is a vital factor in the test. However, a flaw in the material is not always indicated by a low impact value. It may be the cause of high impact value, as when a crack runs parallel to the notched face. It is, therefore, advisable to examine the piece after fracture for any indications of these flaws, and it seems to me that particulars of impact test results should include description, and, if it were possible, the area of the ruptured piece.

After considering all the facts relating to impact tests on notched bars as at present carried out, I am convinced that too much reliance is often placed on them and a much too ready interpretation placed upon the results. A high value is given by a steel in "good" condition, so I am told. But what, may I ask, is a good condition? From what standpoint is it "good"? Is it because a maximum tempering effect has been obtained?

Examples could be quoted from practice of materials whose tests showed low impact values, but which worked satisfactorily over long periods under severe conditions. Until it is better understood what quality is really indicated by the impact value, and what are the causes of variation, it seems not only unnecessary, but unwise to make too much of it. The tests may, in the future, perform an important service, but for the present the whole question should be left in the hands of unbiassed investigators, to determine the real meaning of the tests, and to say what their service shall be. They will probably find their true value as auxiliaries to the tensile test, and then their mysterious value will have become quantitative.

A high impact value is usually obtained when the material is capable of absorbing considerable energy by "cold work," and an examination of fractures shows that cold working does not take place uniformly throughout the material, but is confined to relatively small regions. The distribution in the amount of "work" is surprisingly irregular. A severely worked portion may come almost in direct contact with an apparently unworked portion, the two regions being separated by a crack. It is difficult to trace the distribution of stress and strain which give rise to such a condition, but it seems reasonable to expect that when once a crack is formed it should become a surface of discontinuity with regard to subsequent straining. The position of the first crack and the direction in which it afterwards extends must be largely influenced by slight local variations in the physical condition of the material.

In Fig. 4 is a diagram of an impact test-piece of wrought iron which gave an exceptionally high figure without being broken. You will notice the shape of

the crack. In Micros. 4 and 5 this ruptured area is reproduced, and the foregoing remarks will be seen to be well illustrated. The well-worked material is only separated by a crack from material which is apparently undeformed.

HARDNESS TESTS.—The resistance of material to penetration, as determined by Brinell and Shore tests, gives a convenient check on the quality of the material, and can be readily applied for detecting local differences not indicated by the tensile test. The standard form of the first of these tests involves the determination of the size of permanent impression left after pressing a hard steel ball, of standard size (10 mm.), into a sample of the material at a plane surface, by a

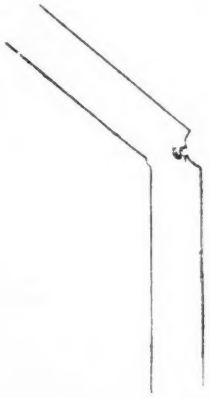


FIG. 4.

stated pressure (3,000 kilograms). The hardness number is taken as the pressure per square millimetre of this spherical area.

In the Shore Scleroscope test the height of rebound is measured by a small diamond-tipped hammer falling from a standard height on to a prepared horizontal surface of the material to be tested. The hardness number which is indicated on the scale is proportional to the height of rebound. In this test a permanent impression is left on the face of the material, similar to but on a smaller scale than that of the Brinell. The reading of the machine depends on the ratio of the amount of energy absorbed in elastic deformation to that absorbed in plastic deformation. In distinction to the Brinell test, the actual pressure applied by the hammer in the Shore varies with the hardness of the material tested.

The two types of test agree fairly well with each other, and in some ranges with the maximum stress from the tensile test. This general agreement, even in a limited range, is useful in suggesting whether the tensile figures denote a proper test or a premature break due to flaws. When, however, we consider the whole range of mechanical properties in steel, the relation between the two tests and between each test and the tensile is not very rigid. The hardness numbers are not proportional to each other, although the Brinell number and maximum stress agree somewhat in this respect.

The whole question as to the real meaning of "hardness" is under consideration by various experimenters, including an important Committee of the Institution of Mechanical Engineers, whose results will probably elucidate much of the vagueness at present attached to this term. The scleroscope hardness, and to a lesser degree the Brinell hardness, no doubt depend to some extent on the elastic properties of the material, but I would take this opportunity of cautioning engineers against looking upon, say, the Brinell as a substitute for the tensile test.

It has already been shown that there is very little agreement between hardness as determined by these methods and wear resistance, or machining properties of material, which properties therefore require special tests. Brinell hardness incidentally is not an infallible guide as to machining properties.

When dealing with hardened steels, the Shore is much more reliable than the Brinell, but really only applies to plane surfaces.

ALTERNATING STRESSES.—The majority of engine parts are subjected to alternating or fluctuating stresses. Many experiments have been made, in extension of the original experiments of Wohler,* to find the endurance of material under such stresses in direct tension and compression, bending, and torsion. These have shown that the maximum safe stress depends on the range and speed of fluctuation, and have given rise to the approximate rule that by adding the " range " of stress to the stress at the maximum end of the " range " we get an " equivalent steady stress." Any such rule is only approximate, and from the special point of view of the aeroplane designer who wishes to get the best possible service out of the material consistently with safety, it is desirable that repeated stress tests should be made on all materials with which he has to deal which undergo such stresses in working. Such tests, however, would take up much time and attention.

Professor Arnold's " Alternating Stress Tests," which can be carried out in a few minutes, combine the effects of alternating stress and shock. The actual stress employed is unknown. The test-bar is fixed at one end, and by forces applied near the other end the bar is bent backwards and forwards through a definite distance, outside the elastic range, at a definite speed. The number of alternations withstood before fracture is taken as a figure of merit for the material. It is, however, noteworthy that the value usually decreases as the maximum stress increases, but this I think would be expected.

The Stanton repeated impact machine applies alternating shocks to a notched test-bar. The energy of blow is derived from the falling of a small hammer. The height of fall can be varied at will, but is kept constant during a test. The number of blows struck before fracture is automatically recorded. The total energy absorbed in producing fracture can be easily calculated. This test would appear to offer useful data to the engineer.

TORSION TEST.—This form of test seems to me to be of particular interest to aero engineers, and I am rather astonished that more attention has not been given to it in its simple form and many possible modifications.

Any shaft which transmits power suffers " torsion," and the natural method of testing the strength of such a shaft or of the material of which it is made should be by torsion. The amount of " torsion " acting at a section of a shaft or test-piece is the total moment, about the axis, of the forces tending to twist one portion relatively to another at that section.

The stresses due to torsion over a normal section of a cylindrical bar are pure shear. For a solid section the shear stress (within elastic limit) is maximum at the skin of the material, and decreases uniformly to the centre where it is zero. The range of variation of stress is reduced by making the section hollow.

The stress-strain diagram for the torsion test shows little, if any, " droop " from the maximum twisting moment. This is due to the absence of local contraction as it occurs in the tensile test. The plastic deformation in the torsion test takes place almost uniformly over the length of the parallel portion. The maximum stress in shear for any material is usually about 60-70 per cent. of the maximum tensile stress for that material, but the ratio is by no means a fixed quantity; hence the necessity for separate tests.

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* Unwin's Testing of Materials.

The total angle of twist to fracture per unit length of test-piece is a good criterion of " ductility " of the material.

The Steels Used in Aero Construction.

In giving a paper of this kind it is totally impossible to deal adequately with the different steels that are now being used in this field, and the best that one can do is to take and discuss a few typical ones. For instance, for case-hardening purposes three or four different steels may be used, sometimes a plain low carbon material, at other times say a nickel or a nickel-chrome steel. When we come to consider high-tension steels many people swear by nickel-chrome steel, but under this term " nickel-chrome " quite a large range of analysis is included; again, some people successfully employ chrome-vanadium steels. To consider air-hardening steels, the term " air-hardening " applies to a range of steels, and the only thing to do is to take a typical one as an example. Looking at matters broadly from this standpoint, I have selected a series of steels which, on detailed discussion, will, I think, give an accurate idea of the large range of composition, coupled with the physical and mechanical properties, which are now available for purposes such as the one under discussion. In the following table will be found a list of the steels with which it is proposed to deal, together with their approximate composition :—

Steel.	Carbon.	Manganese.	Nickel.	Chromium.	Vanadium.	Tungsten.
(1) Case-hardening Steel. Carbon ...	0.12	0.7	—	—	—	—
(Nickel	0.10	0.2	5.0	—	—	—)
(2) Carbon Steel	0.5	0.6	—	—	—	—
(3) High-tensile Steel. Nickel-Chrome	0.35	0.4	3.0	0.6	—	—
(Chrome-Vanadium	0.35	0.4	—	0.6-1.0	0.16-0.25	—)
(4) Air-hardening Steel	0.3	0.4	4.0	1.5	—	—
(5) Stainless Steel	0.3	0.4	—	13.0	—	—
(6) High-Nickel Steel	0.25	0.3	25.0	—	—	—
(7) Magnet Steel	0.6	0.3	—	—	—	5.5
(8) Manganese Steel	1.2	12.5	—	—	—	—

It will be noted that silicon, sulphur, and phosphorus are not included in the table. With regard to silicon, it is now generally admitted that a fair percentage of this element is an advantage, owing to its utility in deoxidising and in assisting to produce sound material. When we come to consider sulphur and phosphorus, it is well that I should emphasise that these elements should be as low as circumstances permit. Whilst discussing these steels from a general standpoint, I must draw your attention to the fact that if you pay the price of a special steel it is necessary that you should submit it to its special treatment if you are to get the value for the money which you have expended. For instance, cases have come before me where expensive steels have been bought and merely forged and dealt with as if they were ordinary carbon steel. In the first place they should not be treated as ordinary steels in the works, because they will require working with special care and can easily be damaged, indeed put into a worse condition than ordinary steel; and in the second place, unless they are put through the special treatment which their constitution needs, the resulting mechanical properties fall very far short from what they should be. It would perhaps be useful to give an example. In Micros. 6, 7, and 8, a definite illustration is given showing the condition in which a 3% nickel steel was actually put into work with a companion photomicrograph showing the condition in which it should exist.

Before discussing the particular properties of individual steels, it will perhaps be as well to explain the changes taking place at the critical change-points which occur in steel by means of a series of heating and cooling curves, since the thermal phenomena is of such fundamental importance.

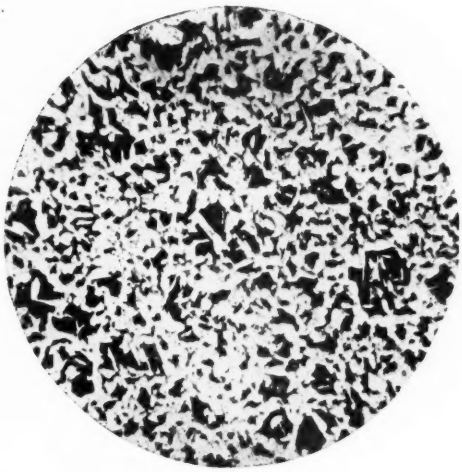
Thermal Phenomena of Steels.

PURE IRON (CARBONLESS).—If a sample of pure iron be carefully studied it will be found that upon heating the specimen up to a temperature of 1,000° C. two distinct arrests of the otherwise regular rise in temperature will have been noticed. The explanation is that fundamental changes have taken place in the material at those arrests, accompanied by an absorption of heat, *i.e.*, energy required to accomplish the changes referred to. Curve A, Fig. 2, indicates diagrammatically the nature and position of these changes. The first arrest at 770° C. marks the appearance of the non-magnetic condition; the second at 930° C. marks an allotropic change in the material, accompanied by change in several properties. On cooling down the arrests are replaced by corresponding evolutions of heat representing converse changes. Pure iron is of little immediate interest to you, and may, therefore, be dismissed after these preliminary comments.

CARBON STEELS.—If, now, a little carbon be added to iron and, say, a case-hardening steel is produced, the heating and cooling curves become modified. Curve B, Fig. 2, illustrates the result. In Micro. 3, which illustrates the structure of a carbonless iron, it will be seen that we have present only *ferrite* (pure iron) crystals, whilst in Micro. 9, illustrating the structure of a case-hardening steel, it will be seen that we have present in small proportion the new dark etching constituent "Pearlite." This pearlite, existing at ordinary temperatures, is a mixure of iron-carbide plates and iron. When a temperature just over 700° C. is reached this constituent changes into a homogeneous substance, *i.e.*, a solid solution of carbide of iron with an absorption of heat resulting in an additional arrest on the heating curve non-existent in the case of pure iron. It will be noticed that the magnetic change still occurs at the same temperature, whilst the allotropic change, Ac3, occurs at a distinctly lower temperature. It may also be pointed out that as the temperature rises after passing the carbon change-point, the areas of solid solution increase owing to the solution of more and more ferrite in the original solid solution areas, and finally, in passing the allotropic change, the whole material becomes a homogeneous dilute solid solution of carbide in iron. On cooling, the reverse changes take place at the temperature indicated in the Curve B, Fig. 2. On reaching ordinary temperature, providing adequate time for cooling has been allowed, the material is as before the experiment.

To take now a 0.6 carbon steel. The microstructure of such material is shown in Micro. 11. It will be seen that the major portion of the material now consists of the constituent pearlite, and thus it might be expected that the carbon change-point becomes still very much more evident and that the other critical points associated with a pure iron are very considerably lowered as regards the temperatures at which they occur. A heating and cooling curve for such a steel is given in Curve C, Fig. 2. It will be seen that the upper critical points have, for practical purposes, been lowered to and joined up with the carbon change-point. It will be noticed that the absorption of heat, synchronising with the formation of the solid solution on heating, takes place at about 730° C., whilst on cooling the evolution of heat associated with the breaking down of the solid solution takes place 20° to 30° lower.

IRON-CARBIDE DIAGRAM.—It was thought useful to include the Iron-Carbide Equilibrium Diagram, and this is given in Fig. 1. Its practical application will now be quite apparent. It will be seen to deal with iron containing carbon from nothing to 5.0%, *i.e.*, it covers the range from iron-carbon steels to pig iron, and diagrammatically presents a mass of useful facts. If the *arrests* during cooling of an infinite number of pure iron carbon alloys covering the range of the diagram from 1,600° C., *i.e.*, well in the liquid state, are plotted, the lines presented will be obtained. This is rather an over-statement, since the lines A.D. and I.D. have had to be confirmed by other methods. It is, however, no part of my present

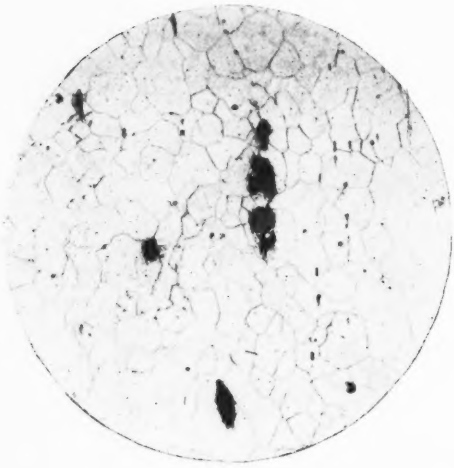


Micro 1. Etched × 100.

Mild Carbon Steel.

Micro 2. Etched × 50.

Illustrating Bend Test.

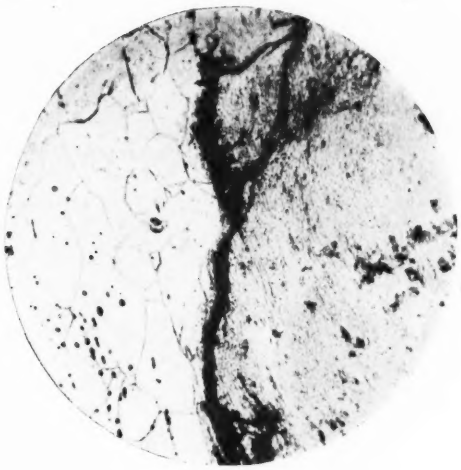


Micro 3. Etched × 100.

Pure Wrought Iron.

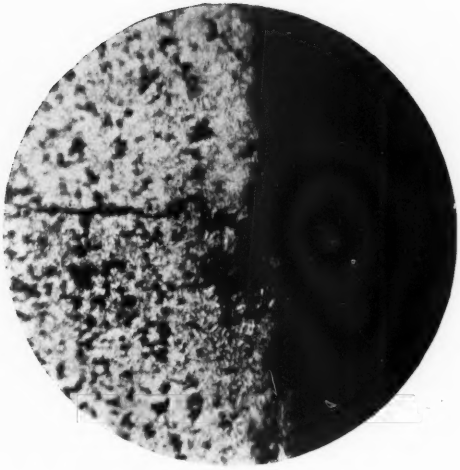
Micro 4. Etched × 25.

Illustrating Impact Test.



Micro 5. Etched × 100.

Illustrating Impact Test.

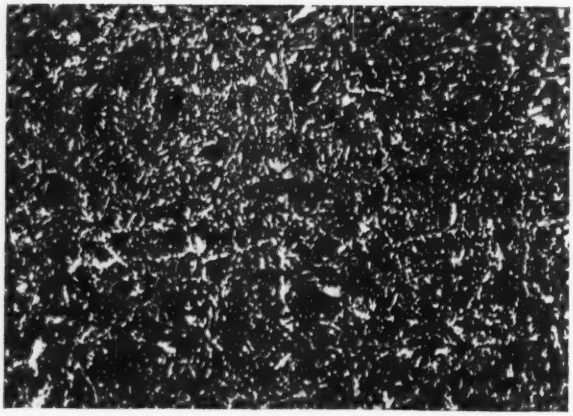


Micro 6. Etched × 100.

Badly Worked and treated Nickel Steel.

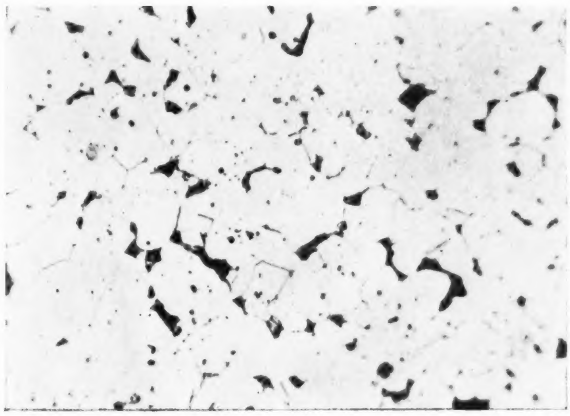
Micro 7. Etched × 100.

Improperly Treated Nickel Steel.



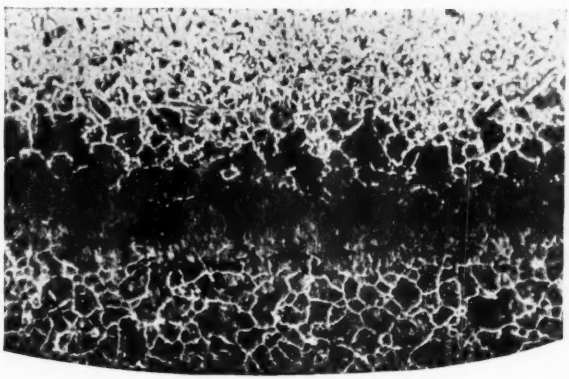
Micro 8. Etched × 500.

Properly Heat-Treated Nickel Steel.



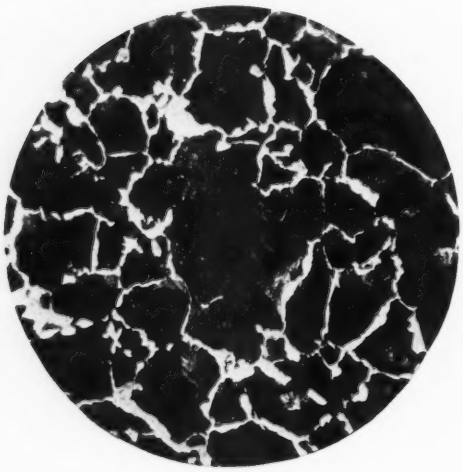
Micro 9. Etched × 300.

Case-hardening Steel.



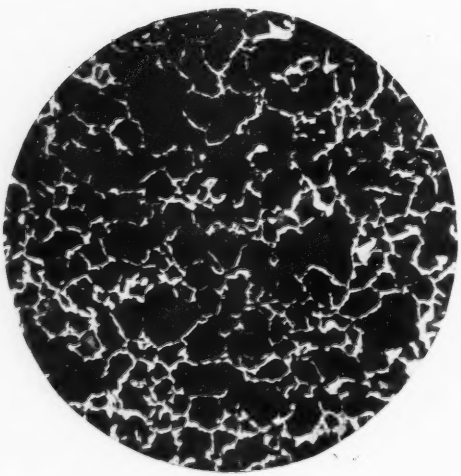
Micro 10. Etched × 25.

Carburised Outer Layers of Cased Specimen (Unhardened).



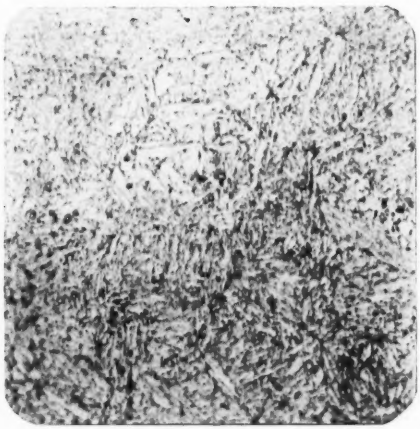
Micro 11. Etched × 100.

Carbon Steel (Forged Condition).



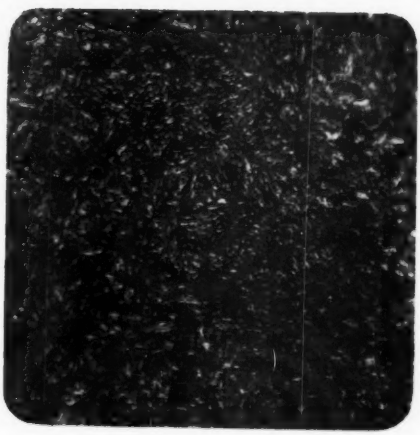
Micro 12. Etched × 100.

Carbon Steel (Normalised Condition).



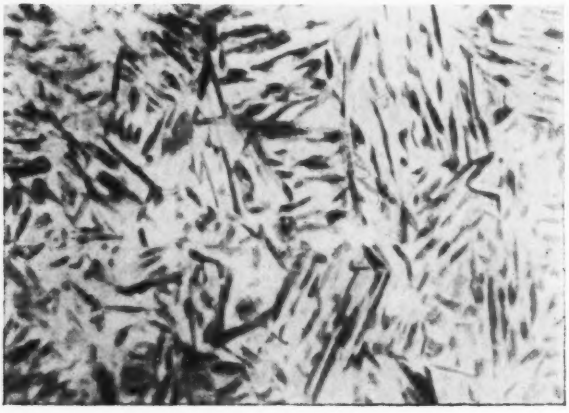
Micro 13. Etched × 500.

Under Tempered High Tension Steel.



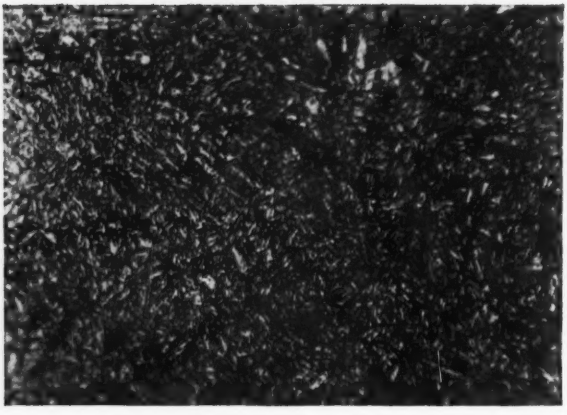
Micro 14. Etched × 500.

Properly Hardened and Tempered High Tension Steel.



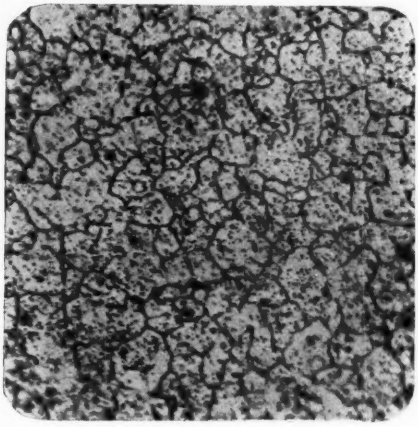
Micro 15. Etched × 500.

Hardened Air-hardening Steel.



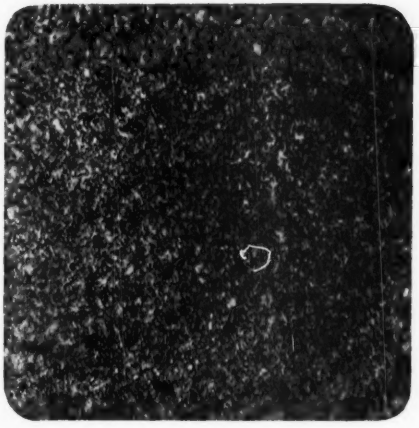
Micro 16. Etched × 500.

Annealed Air-hardening Steel.



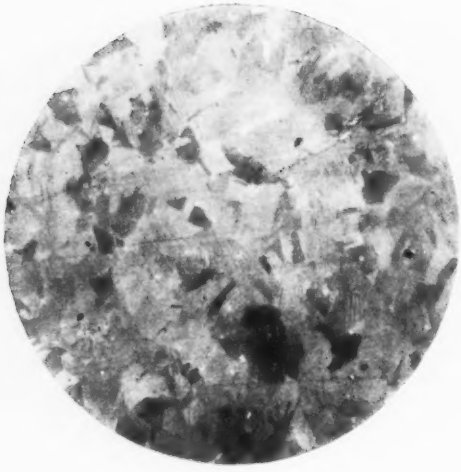
Micro 17. Etched × 500.

Hardened Stainless Steel.



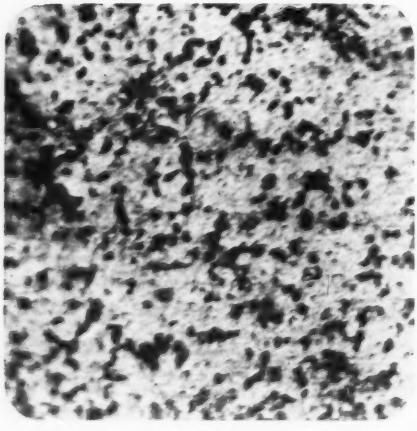
Micro 18. Etched × 500.

Tempered Stainless Steel.



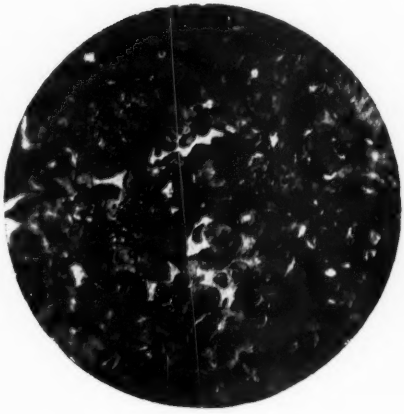
Micro 19. Etched x 100.

Manganese Steel.



Micro 20. Etched x 500.

Magnet Steel.

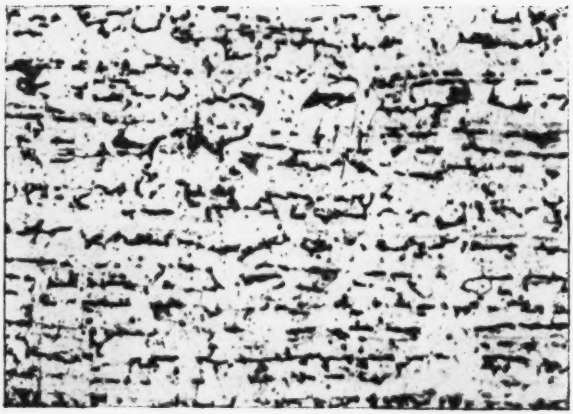


Micro 21. Etched × 50.

Strong Cast Iron.

Micro 22. Etched × 50.

Weak Cast Iron.



Micro 23. Etched × 500.

Sheet Steel.

paper to discuss the theoretical details of this diagram. There are, however, three main lines to which I wish to draw your attention: (1) The line A-B, indicating that by increasing the carbon content the freezing point is dropped from 1,506° C. for pure iron to 1,137° C. for the eutectic, *i.e.*, pure pig iron. This is the explanation of the low melting point of the cast irons. (2) The E-G-I indicating that the solid solution (single constituent) persists to progressively lower temperatures until the material contains 0.9% of carbon. The case-hardening and carbon steels recently dealt with may be considered from this standpoint. (3) The line I-J extending completely across the diagram, which indicates that in the pure iron-carbon series the solid solution breaks down into pearlite at practically the same temperature.

Curves A, B, and C, Fig. 2, will be found to fit in with the equilibrium diagram, always remembering that the latter is built upon the *cooling* curves. It is perhaps wise to here place a caution with regard to the position of the lines shown on the diagram. The position of these lines will be found to be affected by the manner in which the cooling curves are made. In the first place, absolutely correct calibration of the pyrometer must be obtained, and in the second place it must be understood that (1) size of mass, (2) speed of heating and cooling, (3) degree of maximum temperature attained during the heating, all actually influence the temperature at which the physico-chemical changes take place. In illustration of this point, we cannot do better than instance the results contained in a paper given by Dr. Stead, F.R.S., at the Brussels Meeting of the Iron and Steel Institute in 1913. Dr. Stead prepared a number of test-pieces containing practically nothing but iron and 0.90% of carbon. This, as will be seen from the diagram, should have only one arrest on heating and one evolution of heat on cooling; therefore it was an extremely suitable steel for comparative determinations. One each of these bars was sent to contemporary metallurgists and institutions and results obtained for comparison. It may be stated that the figure given for the absorption of heat on heating covered the range from 719° to 746° C., whilst on cooling the change was variously stated from 697° to 719° C. It should be noted that the remarks made in the last paragraph apply even more to alloy steels, such as used in your work, than to ordinary carbon steels. After commending the equilibrium diagram to the careful attention and study of all interested in the heat treatment of steels, I now propose to leave it for the present.

Nickel-Chrome Steels.—If other elements, such, for instance, as manganese, silicon, chromium, nickel, tungsten, vanadium, etc., are added to steel, the position and character of the arrests on heating and the evolutions on cooling such steels are apt to be modified. To take the steels D and E, Fig. 2—the nickel-chrome high-tension and air-hardening ones. The change to the solid solution state with absorption of heat takes place at approximately the same temperature on heating, but on cooling the breaking down takes place at a much lower temperature. Here is the explanation of the value of added nickel and chromium to a steel. It makes the carbon change more sluggish, *i.e.*, more easily controlled and suppressed, and here lies the explanation of the reason why such alloy steels may be hardened in larger masses and here also is one of the explanations as to why such excellent physical properties can be obtained by heat treatments. The added element usually brings about its influence by modifying the chemico-physical properties of the iron and carbide. With the nickel and chromium content of air-hardening steels, the speed of cooling produced by air-cooling is quite quick enough to suppress the carbon change and present the "solid solution" state, whereas water-quenching is only effective more or less with carbon steels, according to the mass employed.

High-Chrome Steel (Stainless).—In Curve F, Fig. 2, will be found the heating and cooling curve of the high-chromium steel. Here the change to the solid solution state does not take place until about 800° C., and consequently if it is desired to harden such steel it must be heated to considerably higher tem-

peratures than ordinary carbon steels. Again, upon cooling (and here it may be pointed out that this is an air-hardening steel), the change from the solid solution state takes place at varying temperatures, according to the maximum temperature to which it has been heated; the speed of cooling also affects the position of the recalescence point. If slow enough, the change takes place soon after 700° C. has been passed.

25% NICKEL STEEL.—See Curve G, Fig. 2. This steel is unique in that the critical points do not occur in those ranges of temperature met with in works practice, providing the composition under discussion is secured. The result is that no arrests or absorption are observed during heating and cooling, and we have, therefore, only to deal with the solid solution state, modified in properties by the presence of 25% of nickel. It will be seen at once that it is this fact which explains the non-magnetic properties of this material.

HIGH-MANGANESE STEEL.—See Curve H, Fig. 2. Here we have a steel which has no arrest upon cooling, *i.e.*, we obtain again the "solid solution," this time containing 12 to 14% of manganese, which, if well preserved, *i.e.*, cooled with sufficient rapidity, is also non-magnetic. Recent researches by Sir R. A. Hadfield have, however, shown that such a steel after tempering (which in this case hardens the steel) does develop a definite arrest in the usual position of the carbon change-point; hence the dotted indication. If the steel be heated up without tempering, ordinary observations would not detect an absorption of heat at the critical point.

Typical Steels Discussed.

In the adjoining table will be found a list of the typical steels, with the mechanical properties which may be expected from them :—

APPROXIMATE MECHANICAL PROPERTIES OF TYPICAL STEELS USED IN AERO WORK.

Steel. Treatment.	Y.P.	M.S.	E%	R/A%	Imp. (Izod).	Remarks.
(1) Case-hardening Steel—						
Carbon as supplied ...	20.0	30.0	30.0	65.0	—	
Case-hardened Core ...	24.0	35.0	25.0	60.0	—	
Nickel as supplied ...	28.0	36.0	35.0	65.0	—	
Case-hardened Core ...	35.0	50.0	25.0	55.0	—	
(2) Carbon Steel—						
Forged or Rolled ...	25.0	42.0	22.0	40.0	—	
Normalised	23.0	40.0	27.0	50.0	—	
(3) High-Tensile—						
Hardened and Tempered...	55.0	65.0	20.0	55.0	35	
(4) Air-hardening Ni-Cr.—						
Air-hardened	90.0	100.0	10.0	35.0	20	
Oil-hardened and Tempered	50.0	55.0	22.0	55.0	50	
(5) Stainless—						
Oil-hardened and Tempered	40.0	50.0	20.0	55.0	55	Rustless.
(6) 25% Nickel—						
Toughened (quenched) ...	15.0	40.0	45.0	50.0	—	Non-magnetic
(7) Manganese Sheet Steel—						
Toughened	19.0	35.0	Bend double.		—	,,
(8) Sheet Steel—						
High-tensile (annealed) ...	20.0	30.0	,,		—	
Oil-hardened and Tempered	35.0	48.0	,,		—	
Low-tensile (heat-treated)	20.0	30.0	,,		—	
(9) Cast Iron—						
Pearlitic	—	16.0	—	—	—	
Ferritic	—	12.0	—	—	—	

To take different types of steels now in detail :—

CARBON STEELS.—Carbon steels are of general interest because they constitute the major portion of steel produced and also incidentally because they afford a means of comparing the alloy steels. Plain carbon steel—and by that term we mean a steel to which nickel, chromium, vanadium, tungsten, etc., have not been intentionally added—is used, to take a particular case, for the production of aero-cylinders. Such steel is generally fairly high in carbon, and will give approximately a tensile strength of 40/50 tons with an elongation of 17-20%. It has from time to time been questioned whether cylinder forgings require normalising, and in my opinion I would say that it is strongly advisable to put them through this treatment to *dissipate* any stresses left from the forging or punching operation. (See Micros. 11 and 12 for change in structure produced.) If the finishing temperature be a suitable one, this precaution may not be necessary, but it is perhaps best as a safeguard. As illustrating generally the properties of carbon steel, one cannot do better than include the series of tests given before the Iron and Steel Institute some few years ago.* In the following tables will be found the tensile test figures obtained from steels (made in this instance by the Acid Bessemer Process) varying from 0.1 to 0.86% of carbon, whilst the manganese varies somewhat between 0.5 and 1.0%. The figures (tensile test-bars .564in. × 2in., heat treatment on 1in. bars) are given for the forged and normalised condition, since such steels are not submitted to the same treatment as alloy steels :—

AS RECEIVED (ROLLED).

Mark.	Analysis: Carbon %	Analysis: Manganese %	Yield-Point. Tons per sq. inch.	Maximum Stress. Tons per sq. inch.	Elongation % on 2 ins.	Reduction of Area %	Y.P. x 100 / M.S.
A. ...	0.10	0.56	19.1	25.9	37.1	63.4	73.7
B. ...	0.27	0.68	26.7	35.8	32.7	58.9	74.6
C. ...	0.29	0.92	26.6	40.9	25.0	46.8	65.0
D. ...	0.32	0.67	27.1	39.9	26.6	47.8	68.0
E. ...	0.44	0.90	30.7	46.6	25.7	48.2	65.9
F. ...	0.50	0.92	30.6	52.2	19.8	42.3	58.6
G. ...	0.70	0.90	34.7	59.0	17.9	34.6	58.8
H. ...	0.75	0.92	36.6	64.2	15.3	30.6	57.0
I. ...	0.86	1.03	40.6	52.6	1.8	3.3	77.2
NORMALISED.							
A. ...	0.10	0.56	18.5	24.8	37.4	59.8	74.6
B. ...	0.27	0.68	25.0	34.3	30.5	51.2	73.0
C. ...	0.29	0.92	25.7	40.8	26.3	53.5	63.0
D. ...	0.32	0.67	25.5	39.3	24.0	44.4	65.0
E. ...	0.44	0.90	28.4	48.1	21.0	41.8	59.0
F. ...	0.50	0.92	30.6	52.3	18.8	38.2	58.5
G. ...	0.70	0.90	35.7	59.4	15.9	29.0	60.1
H. ...	0.75	0.92	35.9	65.4	10.8	17.1	54.9
I. ...	0.86	1.03	39.0	54.5	1.8	2.9	71.6

It might be useful to here discuss the changes which take place in carbon steels on heating and cooling, and as illustrating the general changes I propose, although carbon steels have already been partly discussed from this standpoint, to take two particular steels, viz., a .25% carbon as representing an ordinary structural steel and a .60% carbon as representing the steels used for purposes with which you are familiar. To study a steel of .25% composition, we can advantageously draw a line on the Iron-Carbide Equilibrium Diagram, Fig. 1,

* McWilliam and Barnes.

in the position S-T. Starting from the liquid condition of the steel, we shall find that freezing commences at a temperature of a little under 1,500° C., followed by complete solidification at the line A-D. The mass then exists as a solid solution down to practically 800° C., at which temperature pure iron crystals begin to separate, and continue so to do until the line L-I is reached. Above 800° C. we have the mass consisting entirely of solid solution of carbide of iron in iron. Below that temperature the iron crystals gradually increase in size and number with falling temperature, the solid solution areas becoming richer in carbon owing to their loss of iron, until at the line L-I the mass consists of less than a third of solid solution containing 0.89% of carbon distributed through a matrix of ferrite. On passing through the critical point marked by the line L-I, these remaining solid solution areas break down and become areas of pearlite. The line K-G is usually placed in the diagram, and represents an evolution of heat which is known to synchronise with the appearance of the magnetic properties of iron; it will thus be appreciated that when the steel passes through the temperature of 770-760° C., the ferrite, already precipitated, becomes magnetic. It is also to be noted that this only applies to the precipitated iron, as the iron of the solid solution retains its non-magnetic condition down to the recalescence point line L-I, which marks the change to pearlite in these areas.

To take a 0.60% carbon steel and study it by means of a line U-V drawn across the diagram, we find considerable modification in the temperatures at which the critical changes take place. The initial freezing temperature begins a little lower, and is followed by complete solidification on reaching the line A-D. The whole mass, consisting then of solid solution, cools down as a homogeneous substance to just over 730° C., at which temperature some little iron separates from the solution. Within 20-30°, however, the mass of the steel still consisting of solid solution breaks down into pearlite, and the result is that we have a steel now consisting of pearlite, in which small areas of ferrite occur, as against, in the .25% carbon steel, one consisting of ferrite in which relatively small areas of pearlite occur.

High-Tensile Steels.

When we speak of high-tensile steels we mean alloy steels containing suitable quantities of nickel, chromium, vanadium, etc., which by simple heat treatment may, in suitably large masses if necessary, be put into such conditions that they will give 50 to 70 tons tensile strength, accompanied by a good elongation.

To produce the final condition of a high-tensile steel, the steel must be first hardened with a view to producing a homogeneous solid solution phase and then must be tempered to such a temperature as will leave the steel just in that condition in which it will give the tests required. The special elements present, such as nickel and chromium, enable the hardening operation to be effectively and easily performed, whilst they also, in a similar way, influence and render more complete the control of changes taking place upon heating for tempering. Later in this paper examples are given of the effects obtained from typical steels by tempering. (See page 294.)

Air-Hardening Steels.

Air-hardening steels seem to me to be extremely important, and I venture to predict that when they are more completely understood will be used to a much greater extent than at present. The difference between an ordinary high-tension steel and an air-hardening steel consists in the fact that an air-hardening steel heated to temperatures of 800-850°, and allowed to cool in air, will have great hardness, whereas the hardness so introduced by the same treatment in an ordinary carbon, or even high-tension steel, is in no way comparable. The air-hardening steels largely used at present are somewhat similar in analysis to

the figures given in the table on page 294, and it will be seen that it is the presence of the high nickel, along with the high chromium, which induces the specific property. When we say that by simply cooling in air this steel becomes hard we mean that the rapidity with which the article will cool in air is quite sufficient to cause an almost complete suppression of the thermal change which is responsible for the breaking down of the solid solution into the pearlitic condition. Such air-hardened steel will, on air-hardening from temperatures from 800-850° C., give 100 tons per square inch maximum stress and upwards, accompanied by elongation in the neighbourhood of 10%. Such figures would seem to me to be extremely valuable to the engineer designing parts such as you are interested in, and one cannot but think that as time goes along much greater use will be made of it. In Micro. 15 is presented the structure of this type of steel in the air-hardened condition, whilst the adjoining micrograph, 16, gives a typical example of the structure existing in such steel after tempering. This steel responds to tempering in analogous manner to the high-tension steels, and as indications as to the kind of results which may be expected in the two states, figures are given on page 294.

Stainless Steel.

This high-chrome steel has been selected as one of the typical steels because it represents an entirely new departure. The stainless and non-rusting properties of this steel were discovered in the Brown-Firth Research Laboratory during the time of my predecessor, Mr. Harry Brearley, and since that time its uses for various purposes have developed until the material now occupies an important position amongst special alloy steels. The table on page 294 gives an indication of the composition of this steel, and it will be seen to be a low carbon steel containing 11-15% of chromium. The steel is an air-hardening one, and consequently requires working with care, but as experience accumulates in the handling of it, practical difficulties, even in the handling of considerable masses, will no doubt be eliminated. In the sub-division on hardening and tempering a full set of figures relating to this steel in its various determined conditions will be found. This steel has the decidedly unique property of successfully resisting the ordinary corrosive action of the weather, change from wet to dry atmospheric conditions, organic acids, oxidising influences, and sea-water. As an instance, a polished surface of this steel may be placed in the garden during any period of the year and left for two or three months, after which it will still be perfectly bright and unrusted. It would seem to me that for seaplane work this steel should have quite a large application. It is at present used for aero valves in the place of high-speed steel, and certainly would appear to form a possible material for making stream-line wires, etc. Owing to the excellent mechanical properties which can be obtained from this steel by hardening and tempering—which mechanical properties are accompanied by the rust-resisting properties—it would seem that for structural parts its use will become more general as time goes on. Ordinary picric acid in alcohol, or the nitric acid usually used for etching to bring up the microstructure of steels, fail in this case to make any impression, but by suitable mediums effective etching can be accomplished, with the results indicated in Micros. 17 and 18, which illustrate the structure of the hardened and tempered condition respectively.

Manganese Steel.

Manganese is added ordinarily to steel to make it forge well, *i.e.*, to eliminate oxygen, and to more than neutralise any sulphur which may be present. It incidentally strengthens the steel even when present in quantities much under 1%. The steel we are considering, however, under the above heading is the special high-percentage manganese steel containing 11-15% of that element alloyed with the steel for the special purpose of obtaining those peculiar properties of the

alloy discovered by Sir R. A. Hadfield something like 30 years ago. The influence of added elements upon the carbon change-point has already been discussed, and it is useful to here record that as the manganese content added to steel is increased, the carbon change-point on cooling is depressed so thoroughly that in the case of this special steel it does not occur even by the time that ordinary temperatures are reached, the result being that ordinary cooling really has the effect of producing the hard, unresolved condition of the material. If this steel be quenched from temperatures approximating to 950°, the well-known tough, non-magnetic condition of the material is obtained. In such a condition its structure is as indicated in Micro. 19.

Much research work has been done upon this steel, and it is important to record that whereas tempering softens ordinary carbon and alloy steels, if a sample in the condition just referred to (quenched from 950° C.) be tempered, it actually becomes progressively harder and magnetic. After such treatment the great toughness and non-magnetic properties can only be restored by a further quenching from a suitable temperature. (The mechanical properties of manganese steel sheet have been indicated in the table on page 294.)

To assist the engineer in avoiding needless worry, it would perhaps be well to point out that commercially this manganese steel cannot be machined (although in sheet form it is easily punched and sheared). The best way of dealing with it, if it is to be used, is to forge to shape, water toughen, and then finish by grinding. Its uses need not be more fully discussed here, but it is considered that the material is of sufficient importance to have required some general indication as to its properties and structures. The remarks will, at any rate, not have failed to have indicated that some of the different steels are quite as unlike as different metals.

25% Nickel Steel.

It is, on the face of it, a curious fact that two magnetic materials such as iron and nickel should, when alloyed in certain proportions, produce a non-magnetic alloy.* A steel containing 25% nickel has this and other valuable properties. As previously pointed out, some elements lower the magnetic and carbon change-points in iron, and this particular percentage of nickel lowers them to something approximating to 40° C. The result is that if the material does not reach temperatures much below normal ones, this steel remains in a non-magnetic condition. It should, however, be pointed out that the nickel must be reasonably near 25%, say within a range of 24.5-27.5, for satisfactory results, with the carbon in the vicinity of 0.30%. The manganese should also be kept low. For anyone particularly interested in this steel reference might be made to a compilation recently published.† Microscopically, such steel will, when put in its best condition, consist of polygonal grains or allotrimorphic crystals of solid solution of carbide of iron and nickel in iron containing nickel, with the structure similar to that contained in Micro. 19. The mechanical properties of this steel are of similar order to the manganese steels (tests are given on page 294), the distinctive feature being the low elastic limit and a slightly lower modulus of elasticity.

With regard to this steel, our experience has been that a little chromium has several advantages. In the first place, it seems to improve the machining properties without deteriorating the non-magnetic properties, and incidentally assists in preventing any precipitation of the carbon in the free condition.

These high-nickel steels, particularly when they contain percentages from 27-32, are claimed to be rustless, and are used considerably for valves, boiler tubes, etc. This rustless property tends, however, to be somewhat fugitive, and

* Noted by Professor Hopkinson in 1889.

† "Invar and Related Nickel Steels."

such steel is apparently losing favour in that direction, especially in view of the introduction of the high-chromium " stainless " steel. A good deal of research work in this field will lead to a more complete understanding of its properties.

Magnet Steel.

The production of good magnets has been, and is, quite an important question, and a few remarks may, therefore, be in place. The analyses of numerous makes of magnets, British and non-British, indicate that there seems to be a fair unanimity of opinion with regard to composition, and it would appear that a steel containing 5.0-6.0% of tungsten is favoured with a carbon content of 0.60-0.80%. The condition of the steel produced by hardening is clearly the vital consideration in the production of magnets, and it was thought of interest to include a micro-structure of a Continental magnet which had not only a high reputation, but which gave excellent results in a check determination of its properties. The structure will be found in Micro. 20, and it will be seen that a fair separation of troostite (dark etching constituent) had taken place during the final hardening. (The Brinell reading of this specimen gave 2.40 impression, 652 hardness number.)

The requisite properties for permanent magnets are high remanent magnetism and high coercive force. These qualities cannot both be raised to the maximum value at the same time, and therefore the first has to be sacrificed to some extent in order to obtain the best value in the second. Coercive force becomes of relatively greater importance the greater the air space included in the " magnetic circuit " and the more severe the conditions become in use. Also vibration and variation of temperature tend to decrease the intensity of magnetisation, and the good value of coercive force is necessary to counteract these effects. As just mentioned, special care must be taken during the hardening of magnet steel if it is to be brought into its proper condition. With suitable treatment a 5% tungsten steel should be made to give a coercive force 55-65 c.g.s. units and a remanent flux density of about 10,000 c.g.s. units.

Sheet Steel.

A considerable tonnage of sheet steel is employed in aero work, and a few words may, therefore, be in place. The supply of sheet steel possessing definite physical properties has been brought very much to the front of late, and there is admittedly much to be learned both by the maker and user. The present method of manufacturing clips, etc., from such steel is worth looking into. These are usually bent to various complicated designs in the cold state by means of the hammer and vice. In order to stand this rough usage, the steel must be supplied of such properties that it will withstand bending double in any direction over itself; it is quite possible to imagine a man who is bending a clip over to a template bending it too far and then having to bring it back again, possibly more than once, producing extreme local cold work. As to how such a practice may deform the structure locally, micrographs illustrating our remarks on bending tests will indicate. Considerable local strain is put upon the metal. The steel used for such purposes has usually a low tensile strength, say 28-30 tons. A higher class of sheet steel is being used containing nickel which gives approximately 35 tons tensile with a considerably higher yield-point than the mild steel. A drawback against this latter steel is that there is some difficulty in working it. It will be noticed that the structure is a very small one, but that it essentially consists of pearlite and ferrite.

As the demand for lightness with strength is such a vital matter, it is quite easy, from the steel manufacturer's point of view, to supply sheets which, when heat-treated, will readily give a tensile of 50 tons, combined with a fairly good

elongation, and sheet capable of tensiles up to 100 tons can be supplied, providing that sufficient care is taken by the aeroplane builders to treat this steel in a very careful manner. I would suggest that whenever high-tensile sheets are required the parts should be pressed in the machine, and in this way the possibility of over-bending, accompanied by the alternate bending, will be eliminated. For those parts which cannot be pressed in bulk, it is suggested that a nickel-steel sheet be used which will stand bending over double and which, when suitably heat-treated after bending to shape, will give a tensile strength of upwards of 40 tons, with a corresponding high yield.

A considerable number of complaints have come to my notice in the past of failure during the bending of this sheet steel, and this has been largely due to the fact that the edges, after shearing, have not been properly dressed, more often than not even small cracks remaining. This results in a ready local accumulation of stress with subsequent local failure. Especially does this occur when the pieces have been roughly cut by means of a chisel.

Welding.—This question of small parts made from sheet steel raises the subject of welding. To my mind welding in any form should be looked upon with disfavour in connection with aero work. The skilled welder even may think that he has made a good job of a weld, but it is usually almost certain that a metallographist could demonstrate that he has not done so. The drawbacks are twofold. First the material has to be put into a bad (most likely burnt) condition in the neighbourhood of the weld, and secondly, unless the welder is a perfect artist, he leaves oxides lying in the path of the weld. One does not want to condemn welding in a general way, and whilst welding is, in other fields, a useful process, it seems to me to be a little too risky in aero work if you can possibly dispense with it.

Steel Tubing.

It would be useful if contributions could be made to the discussion with regard to the manufacture and properties of steel tubing by those with special experience. Much work requires to be done in this field, and there is much to be learnt both by manufacturers and users.

Cast Iron.

Since cast iron is used for cylinders and pistons, a few words are probably in place. This material can be conveniently looked upon for the purposes of our comments as a steel matrix containing silicon in solution which is cut up by graphitic plates and free phosphide. If cast iron is suitably made it is strong, and has other properties which experience has shown make it useful for the purposes for which it is used. Micro. 21 illustrates the structure of such material, and it will be seen that the matrix consists of pearlite. If the iron is cast in a too soft condition the structure shown in Micro. 22 is obtained, and then we have a silico-ferrite matrix of much less strength than the pearlitic matrix to which reference has just been made.

It is well worth the while of engineers to look into this question of composition, particularly with a view to obtaining a suitable silicon content. If the silicon be too high, a weak iron is obtained, whilst if it be too low the fins, for instance, of cylinders will consist of "white" iron and be much more easily broken. A variation in composition without a corresponding modification in feeding arrangements would lead to unsound and unreliable castings, and, therefore, whatever else is arranged, compositions, when once selected for a particular purpose, should be consistently maintained.

Armour.

The question of armouring aeroplanes is a live subject, and one giving much scope to the metallurgist. Unfortunately, this is not a subject for open discussion at the present time.

Forging and Drop Stamping.

When considering the question of the work that has to be put upon steel, it almost seems a platitude to say that the steel must be well soaked and forged at a proper temperature; yet experience teaches us that it is well worth emphasising on every possible occasion since so much trouble has arisen, and is arising, from this cause. The alloy steels, too, should be heated up slowly and with care, particularly from the cold until they are thoroughly warmed through, and any experienced forgeman will confirm the great advantage of properly soaking the material. Internal rupture of the material is quite possible with under-soaking, whether it be due to ignorance or to rushing production. There is another aspect of the forging question, *i.e.*, different parts may be forged in different ways, and there is probably no better example than a crankshaft. In this connection not only must it be considered as to how much work may be locally put on the material, but also where the forging operation will leave the central, less homogeneous, and less reliable portion of the material. The method of stamping aeroplane cranks by which the bloom is forged to crank shape in two or three heats has been advanced as a desirable method. This, however, like most other processes, is open to criticism. To satisfactorily produce a crankshaft by this means, the temperature of the material must be judged with great care, as, owing to the severe punishment which the steel locally receives, it may even be permanently injured. The forging of the different items really requires a paper dealing specially with that aspect of the matter. One point, however, which does seem to me important and worth emphasis is that the several parts shall receive a requisite amount of work and a minimum of re-heating upon parts not subsequently re-worked, the work being put upon the material at suitable temperatures.

My friend Mr. Stubbs* has already dealt recently with drop stampings for the automobile industry, and his remarks *re* the technique apply obviously equally well to aero work. As he says, "drop forging may be defined as the art of causing metal in the hot and plastic state to flow into cavities formed in one or both of a pair of dies." Obviously, then, the art will depend largely upon the skill of the drop-stamper in determining the correct temperature at which to work his steel. This temperature must necessarily vary with different steels, some permitting liberties to be taken whilst others will not, and also with the size and configuration of the stamping. Some of the steels used in aero work need extremely careful handling if drop stampings are to be made from them, whilst there are other steels which probably the drop-stamper will be wise to avoid. It will be appreciated that if certain designs of parts are to be produced, owing to the unavoidable difficulty as regards configuration, some steels are probably immediately excluded, and here is only one direction where the drop-stamper and designer can well afford to consult and to take advantage of the steel-maker's experience. Many faults are put down to the steel which can most likely be attributed to the stamper's lack of familiarity with the idiosyncrasies of the materials with which he is dealing. Talking of the designer reminds me that much time, money, and subsequent worry can be saved if that original sinner will remember that the steel-maker, drop-stamper, and aero engine builder are frequently the heirs of trouble which he not infrequently bequeaths to them. Simplicity in design, absence of sharp corners, acute angles, deep thin ribs, acute

* Institution of Automobile Engineers, 1915.

bends, etc., are points perpetually held before him as things he should not perpetrate, and be it said there are some signs of improvement. The effect of over-heating for drop stamping has already been pointed out on several occasions, and the evil effects do not require any further emphasis. Working too cold is also undesirable, since it produces stresses in the material which may lead to subsequent trouble.

Before leaving the question of drop stampings a word might be said with regard to the heat treatment. Frequently a stamping will be of very unequal thickness in its drop-stamped condition, whereas after machining it will have attained probably fairly uniform thickness. In such cases it would seem to me desirable that the heat treatment should be done after rough machining, as otherwise it is extremely likely that the material in the heavy part of the forging will not have been put into the same condition as that in the portion of lighter section.

Normalising and Annealing.

The terms " normalising " and " annealing " are loosely applied to a variety of operations in a steel works. Steel in the forged or rolled condition is in a strained state, and in order to destroy the stresses remaining from these operations it is necessary to re-heat the material. This may be accomplished by either of the operations under discussion, but in addition to the necessity for removing stresses it is often necessary to also soften the material by entirely modifying its structure. The actual operation of normalising consists in heating the steel above the Ac_3 point and allowing to cool in air free from draughts. Annealing is a much more thorough treatment, involving a knowledge of the behaviour of the various steels under varied conditions. The original method of annealing was to heat the steel to a high temperature, maintain that temperature for a varying time, and cool very slowly. This is quite satisfactory for many steels, but in some of the alloy steels better results are obtained by heating to temperatures below the carbon change-point and cooling in air or in the furnace. It may be of interest to include the I.A.E. Steel Research Committee's definition of these terms :—

> NORMALISING.—Normalising means heating a steel (however previously treated) to a temperature exceeding its upper critical range and allowing it to cool freely in the air. The temperature shall be maintained for about 15 minutes, and shall not exceed the upper limit of the critical range by more than 50° Centigrade.
>
> ANNEALING.—Annealing means re-heating, followed by slow cooling. Its purposes may be :—
>
> (*a*) To remove internal stresses or to induce softness, in which case the maximum temperature may be arbitrarily chosen.
>
> (*b*) To refine the crystalline structure in addition to the above (*a*), in which case the temperature used must exceed the upper critical range as in normalising.

Hardening and Tempering.

For many centuries swords, knives, and suchlike articles have been hardened and tempered, and the " finely-tempered " sword, an article in past ages much in demand in times such as these, was produced long before pyrometric observations and the science of metallography had explained the fundamental principles of such processes. In considering the hardening and tempering of steel parts for aero and automobile work, we are only following the example of the time-worn procedure of the old cutlers and armourers. Now, be it noted, that a novice would never in the old days have been permitted to harden and temper, only the

highest skill and experience permitting the production of the desired results, and to-day the same remarks apply. There is quite enough scope in the art of hardening and tempering small parts, such as used in this and other branches of engineering, to justify the growth of a class of operators with brains to understand and skill to execute the treatments required to obtain those excellent qualities which skilful hardening and tempering will induce. Bearing on this, it not infrequently happens that small parts are required in which one portion will have to be finally tempered and tough whilst the other is quite hard, and even more difficult requirements can be met providing the development of the necessary technique in the operator is insisted upon. In the whole range of steel metallurgy there is no direction where careful study and good technique can produce more valuable results.

The operation of hardening consists of quenching after raising the steel to temperatures above the critical point, at which it will exist as solid solution. In dealing with parts made of .3 to .4% and under of carbon, it is not only sufficient to raise to a temperature at which the solid solution areas are formed, but the temperature must be attained at which the ferrite has also disappeared owing to its solution in the solid solution. The desirable temperature, therefore, for hardening is that at which you quench the steel as a homogeneous solid solution. If you do not go to temperatures sufficiently high for this, you simply obtain patches of ferrite, which reduce the effectiveness of the whole operation of hardening and tempering. Generally, temperatures from 800-850° C. are quite sufficient for this operation, but the actual temperature depends upon the steel, and in this connection it is particularly important that the operator should appreciate the temperatures at which the changes take place in the given steel. It might be well to here caution against the use of too high quenching temperatures.

(1) If the temperature is taken too high, an undue brittleness is introduced into the steel.

(2) Quenching from higher temperatures naturally means greater internal stresses.

(3) If too high temperatures are employed, the quenching medium, if of limited volume, becomes heated to a greater temperature during the quenching operation, and is, therefore, less effective in quenching the material, the speed at which the article passes through the critical zone being most important.

Whilst discussing hardening, it might be well to point out that rapid heating should, for obvious reasons, be avoided. It will be remembered that in speaking of " factors of safety " the question of hardness cracks was brought in, and it might here once more be emphasised that carelessness in hardening and tempering may vitiate the results of most excellent insight and care in design. It will also be fully appreciated that in the hardening operation sharp corners, angles, etc., are a disadvantage just as in other items of process to which reference has been or will be made.

With regard to the medium employed for hardening, that is determined by the character of the steel, and may either be water, oil, or air.

The tempering operation, resulting in the degree of softening required, depends upon the particular temperatures to which the hardened part is submitted, the re-heating causing the breaking down of the hardened state of the steel. There is not time in this short paper to discuss the theory of the matter, and it will perhaps be well to now give typical examples of what the effect of tempering at various temperatures may be on specific steels. It is proposed to take as examples a carbon steel, a nickel-chrome steel, and a high-chrome steel. Particulars of series of such heat treatments are given in the adjoining tables,

which have been prepared from work done in the Brown-Firth Research Laboratory :—

CARBON STEEL.

Size of test-piece, 2ins. × .564in.

Size of piece treated, 1in. round × 8½ins. long.

Treatment.	Y.P.	M.S.	E%.	R/A%.	Izod Impacts.
As annealed	16.93	30.95	31.0	49.7	26, 21, 25
½ hr. 850° C. and cooled in air					
W.H. 850° C. Temp. Nil	—	50.47	15.0	33.47	19, 23, 16
,, 1 hr. 100° C.	—	49.59	15.0	40.51	23, 19, 22
,, 1 hr. 200° C.	—	49.44	14.0	34.91	16, 18, 25
,, 1 hr. 300° C.	—	46.55	17.5	39.13	21, 17, 28
,, 1 hr. 400° C.	—	51.26	16.75	39.13	21, 27, 33
,, 1 hr. 500° C.	31.51	47.21	20.0	49.7	34, 35, 31
,, 1 hr. 600° C.	30.12	42.3	25.5	61.48	56, 68, 53
,, 1 hr. 700° C.	24.41	36.27	33.0	69.77	84 : 86 + 28 : 82

HIGH-TENSION STEEL, NI-CR.

Size of test-piece, 2ins. × .564in.

Size of piece treated, 1in. round + 6½ins. long.

Treatment.	Y.P.	M.S.	E%.	R/A%.	Bri-nell.	Izod Impacts.
A.H. 800° C. Temp. Nil	41.37	56.46	19.0	49.70	3.9	18, 22
,, 1 hr. 600° C.	37.02	46.49	24.0	63.65	4.3	85, 85
,, 1 hr. 650° C.	33.08	43.73	26.75	65.76	4.4	86 10 : 86 11
O.H. 800° C. Temp. 1 hr. 300° C.	82.73	89.61	13.0	54.60	3.2	4, 4
,, 1 hr. 400° C.	72.09	78.98	15.5	58.11	3.4	14, 13
,, 1 hr. 500° C.	59.88	64.83	19.5	62.58	3.6	54, 55
,, 1 hr. 600° C.	49.32	55.15	23.0	63.65	3.9	71, 75, 74
,, 1 hr. 625° C.	46.28	52.89	24.0	65.76	4.0	78, 77, 78
,, 1 hr. 650° C.	39.79	50.54	25.5	65.76	4.05	81, 79, 79
,, 1 hr. 675° C.	35.45	51.71	23.0	58.11	4.0	59, 63, 65
,, 1 hr. 700° C.	34.66	53.54	23.0	47.15	4 0	56, 55, 53
,, 1 hr. 725° C.	40.18	58.69	19.0	36.34	3.9	43, 40, 48
,, 1 hr. 750° C.	40.18	59.33	17.0	33.47	3.9	23, 20, 21
W.H. 800° C. Temp. 1 hr. 600° C.	51.33	56.64	24.0	64.71	3.9	73, 76
,, 1 hr. 625° C.	47.59	53.06	24.0	65.76	4.0	86, 85
,, 1 hr. 650° C.	44.12	51.80	26.0	67.80	4.2	85, 86

STAINLESS STEEL.

Size of material, 1⅛in. round × 7½ins. long.

Treatment.	Y.P.	M.S.	E%.	R/A%,	Bri-nell.	Izod Impacts.
A.H. 875° C. Temp. 1 hr. 500° C.	70.9	85.9	13.0	40.5	3.12	
,, 1 hr. 600° C.	44.5	53.6	21.0	59.2	3.9	69, 73, 69 & 59
,, 1 hr. 700° C.	31.6	45.2	26.0	64.6	4.25	86 + 3, 86 + 13 & 86 + 17
,, 1 hr. 750° C.	29.5	43.9	28.0	63.6	4.25	86 + 16, 86 + 26 & 86 + 25
,, 1 hr. 800° C.	31.6	43.3	27.0	64.7	3.7	33, 33, 43
O.H. 875° C. Temp. 1 hr. 500° C.	72.6	90.5	8.0	18.23	3.1	
,, 1 hr. 600° C.	39.4	52.0	20.0	56.9	5.9	72, 67, 76
,, 1 hr. 700° C.	34.8?	47.1	25.5	63.8	4.07	84, 86, 86
,, 1 hr. 750° C.	39.4	44.1	27.0	66.3	4.2	86 + 33, 86 + 29 & 86 + 42
,, 1 hr. 800° C.	59.1	71.6	4.0	11.7	3.67	24, 24, 18

Treatment.		Y.P.	M.S.	E%.	R/A%.	Brinell.	Izod Impacts.
W.H. 875° C. Temp.	1 hr. 500° C.	70.9	90.2	12.0	34.2	3.1	
,,	1 hr. 600° C.	40.3	53.9	22.0	59.8	3.87	83, 80, 80
,,	1 hr. 700° C.	29.5	45.8	25.8	64.7	4.1	86+35, 86+34 & 86+24
,,	1 hr. 750° C.	30.0	43.7	27.0	65.2	4.22	86+12, 86+15 & 86+19
,,	1 hr. 800° C.	30.6?	48.0	18.0	59.2	4.3	86+39, 86+34

These figures will illustrate the different responses which these types of material make to similar heat treatments.

Before leaving this part of the subject, a word or two might be said on air-hardening. Owing to the composition of such material, the critical changes become so sluggish that merely heating up to a certain temperature and cooling in air is sufficient to effectively produce the hardened condition. This property also allows of larger objects being hardened throughout and, incidentally, owing to the less drastic effect, does not so easily result in cracking. Air-hardening steel after forging and rolling is already in such a hardened state that tempering alone will put it into fairly good condition. It might also be pointed out that since billets and forgings, after working, are hard, they should only be heated slowly and with the utmost care; rapid heating causes a good deal of trouble.

Machining Properties.

There are a few difficulties which arise in connection with machining. For instance, if the tensile of the material exceeds 60 tons per square inch, machining becomes very difficult, and if much higher tensiles are required it is necessary to heat treat the material afterwards. (For parts which have to be hardened after machining the material can be worked in a soft condition with accuracy and speed.) For complicated parts this introduces difficulty owing to the distortion, etc., produced in final heat treatments, but these troubles are now being largely diminished, and, after all, much depends upon the steel chosen and methods of working. One interesting point which I think is fully deserving of discussion is the apparent fact that the Brinell figure does not necessarily indicate the ease with which the material will machine. When one considers actually what takes place during machining, *i.e.*, the manner in which local stresses and deformation, ending in the complete separation and rupture of the turnings, take place, it certainly would appear that there are steels and conditions of steels with which it could not be expected to have a definite indication of the machining properties from the Brinell hardness numbers (manganese steel, in the toughened condition, being one abnormal case in point). Sometimes trouble is encountered owing to the material being too soft; for instance, in the machining of aero cylinders the material will drag if the Brinell hardness number be below a certain figure, but I think a good deal of this difficulty can be got over by modifying the angles to which the cutting tools are ground. The speed, too, at which the work is done in such a case is a factor. If, however, in the case of carbon steel, the Brinell is studied, it will not be proved unreliable, as experience shows that with such steels a careful check of the hardness by this method will result in solving the trouble. It is interesting to record that with the mildest steels the actual size of crystal and amount of inclusion influence the machining properties of the material.

Case-Hardening.

The process of case-hardening is employed when it is desired to obtain a hard shell supported on a tough interior. There are several parts in which you are interested in your particular work for which this process is used, and I

therefore propose to briefly discuss the principles involved. The properties ultimately required in the core largely determine the type of steel which you may select, although the quality of the hardened case also determines the material available. For instance, if the hardest of cases is required, it would not be desirable to use a case-hardening steel high in nickel, since experience indicates that the presence of nickel measurably reduces the maximum hardening effect which can be obtained in the case. It will, therefore, be necessary to carefully study the general properties required in the final article before a decision is made with regard to the steel to be employed. When the objects are ready for case-hardening they are packed in a suitable carburising medium and raised to temperatures of 900-950° C., at which they are maintained for the time necessary to give the required depth of case. This period may be anything from 2-12 hours.

To discuss the case-hardening of a carbon case-hardening steel. From remarks made earlier in this paper it will be apparent that when a temperature of 900° C. is obtained the steel of which the article consists will structurally have become a homogeneous dilute solid solution of carbide in iron, the iron then being in the condition in which it will dissolve very much more carbon than that originally contained in it. The result is that on being placed in such a condition in contact with the carburising medium, solid or gaseous, carbide of iron is synthetically produced where the article and the medium are in contact, and then this carbide is systematically dissolved and diffused into the steel. Since, if equilibrium were allowed to be attained, *i.e.*, if the steel were allowed to dissolve as much carbon as its condition at such temperatures would permit, it would become to all intents and purposes a steel of high carbon content throughout its thickness, it is necessary to determine by experience with the particular materials employed how long exposure is required just to produce the requisite thickness of case. After the required time at the carburising temperature the steel obviously consists of a shell of solid solution high in carbon, with a core of solid solution low in carbon, and so on cooling down we have different changes in structure in the two materials, pure iron (ferrite) crystals making their appearance in the core, whilst the case persists as a homogeneous solid solution (apart from the separated excess carbide existing in it) right down to the carbon change-point. When cold, as time has not permitted the carbide to diffuse into the core of the mass, we have in the central portion approximately the original structure. Experience teaches us that the carburising operation leaves the core in a coarse condition, and it is, therefore, desirable to re-heat the articles to a temperature of 850-900° C., followed by cooling in air or by quenching as a refining operation.

Having carburised the articles, they now require hardening, and I do not hesitate to say that this hardening operation requires all the possible skill available in the operator. It will be clear from previous discussion that when the carburised articles attain a temperature of 730-740° C. the whole of the case, but only the pearlite areas of the core, become solid solution, *i.e.*, capable of producing the hardened condition of steel. It will thus appear that there is a theoretical temperature which, if attained throughout the article, would give a hardened case with a minimum section of area of hardened material in the core; if this temperature is exceeded, the solid solution areas in the core grow progressively in size owing to the gradual solution of the ferrite into the already existing solid solution areas, and at a temperature of a little over 800° C. the whole of the core will have again become a homogeneous solid solution capable of being hardened. I think it will be obvious from this that the core may be trapped by quenching in very different conditions; it may consist either, with lower quenching temperatures, of a matrix of soft ferrite with hardened steel "inclusions" embedded in it, or of a matrix of hardened steel with soft iron "inclusions." The fracture and, incidentally, the mechanical properties of the core will vary widely with the quenching temperature, and once you have decided the kind of core you want, only the most careful workmanship, coupled with an

appreciation of the underlying principles, can systematically produce the required result.

Before leaving this part of the subject, it would be well to emphasise that there is a danger in too low quenching temperatures. One of the great troubles of people who practise the case-hardening process is the appearance of "soft spots." One of the most definite and obvious explanations of "soft spots" is that the material has been quenched at so low a temperature that at certain centres during the quenching operation the breaking down of the solid solution is permitted to occur. When this happens, if the case-hardened article is etched, you will have the "soft spots" definitely shown up as dark etching areas.

CASE-HARDENED GEARS, ETC.—Whilst dealing with case-hardening, it would perhaps be well to make a few comments on gear wheels, etc.; the stresses in such parts are of interest. The teeth are submitted to (*a*) a bending action, producing direct tension and compression; (*b*) local surface pressure and abrasion. The body is submitted essentially to torsion, which produces shear. The loads to which the gear is submitted are all fluctuating and frequently applied with shock. The respective merits of soft and tough core as against uniform hardness throughout seem to form a perennial subject for discussion, but it would appear to me that the comparative merits depend essentially on the amount of shock which has to be withstood. If the parts were not submitted to shock and everything ran perfectly smoothly, then it would be clear that the properties required in the material are surface hardness (resistance to wear) and resistance to fluctuating stresses. Such conditions are best fulfilled by uniform hardness. If gears could be perfectly designed and machined, and if they retained their shape absolutely during hardening and there were no change of gear or other sudden applications of load, then shocks would not require to be dealt with. Unfortunately, it is impossible to attain these ideal conditions in practice. The slight distortion in the shape of the teeth is sufficient to change smooth running into a series of shocks, particularly whilst the wheels are new. Now to consider the question as to whether a soft core does help to withstand shocks, it has been suggested that the capacity of the interior for deformation in such a case is not brought into play until the skin has been deformed beyond its elastic limit, *i.e.*, until the part has been ruptured. In my opinion this is not a correct view of the case. The core, having a lower elastic limit, reaches that value before the skin becomes permanently deformed. After this point the deformation produced is greater than would be produced in a wheel of uniform hardness, the stresses in the skin still being within the elastic limit. It should also be remembered that the stresses produced in a thin hollow case for a given deformation are less than would be produced by the same amount of deformation in a solid body of the same material and external shape. With a very thin casing the soft interior takes up the bulk of the energy of the shock and the hard outside takes up the deformation without becoming very severely stressed. This deformation may, however, be of a permanent nature, involving residual stresses within the elastic limit in the casing. I think it will be clear that this argument points to a soft core as being the best for gears which have to withstand shocks, and I shall be interested to hear the result of your experience in this matter.

When we come to discuss a case-hardened ball race we find that we have to consider not so much the question of shock but the resistance to fluctuating stresses. For this purpose a too soft core would be a disadvantage and the maintenance of a higher standard of strength should be taken into consideration during manufacture. It might be claimed for a soft core in this case that a slight yielding in the ball would tend to increase the area of contact between the ball and race and so decrease pressure per unit area. I would, however, suggest that slight modification in the design of the ball and race with a view to modifying the relative curvature of the surfaces would give the same results without stressing the material beyond the elastic limit at any point.

Specifications.

The war has emphasised the necessity for standardisation of materials and parts generally, and particularly does this apply to automobile and aero engineering. Before the war the steels employed were legion, but as the consumer was actually only getting one quality for each purpose, little trouble was experienced. The war, however, necessitated such large supplies that he had to obtain his supplies from several sources, and consequently got very different steels for the same purpose, with disadvantages far too apparent to require discussion. I have already insisted upon the necessity of the engineer understanding the characteristics and possibilities of the steels with which he has to deal, and it will obviously be an advantage to him if the types of steels are limited to a selected number of the most suitable, so that he will be enabled to focus his attention and have a much greater chance of acquiring that necessary intimate knowledge which his work demands. The Institution of Automobile Engineers have a Committee (of which I have the pleasure of being a member) which has selected and defined the compositions of a number of suitable steels, and the same Committee is now at work thoroughly investigating their properties. I know you aero experts are watching this work with considerable interest, and I feel sure the results will be invaluable to you. At the same time it should be remembered that specifications cannot deal with all the useful steels, as there are many, specially made for certain purposes, which are necessarily difficult to so include; furthermore, steel-making is one of the most progressive arts, and from time to time no doubt new steels will come along possessing properties of sufficient merit to give them place. I would, therefore, suggest that a progressive branch of engineering like yours should not be unduly fettered by too rigid standardisation, and have faith that if you will only express your needs the steel industry will take a pride in responding to them.

Bearing upon specifications, the analysis is not all sufficient, and, indeed, satisfactory mechanical proof tests are not in themselves complete, since one of the greatest troubles which you engineers have to consider consists in the occasional local unreliability. The whole process of production requires the most careful study and attention, and it is up to the steel-maker to see that all the factors entering into the complete reliability of the ultimate product receive the attention which they merit. To begin with, the actual melting and refining operations require complete understanding and control; the actual casting temperature is a vital factor in determining the relative position of unsoundness, etc., in the resulting ingot; the shape of the ingot is also very important. These three items have a great bearing upon reliability and freedom from segregation, which two items I would suggest are of extreme importance. When we come to the actual working down of the steel here again the technique is so important that if the steel is placed in indifferent hands the best of material may be rendered indifferent, or, indeed, may be ruined. The speed with which the ingots or blooms are re-heated, together with the actual temperature at which the material is soaked before working, are important, and it is not necessary for me to point out that there are factors in the actual rolling and hammering operations which require detailed experience if those sections of process are to be satisfactorily dealt with. Incidentally, the actual temperature at which the material is finished in the mill or at the hammer has a bearing on the results. I think I have said enough to indicate that, over and above the existence and execution of well-devised specifications, the degree to which the art of steel manufacture is successfully practised is really the ultimate factor in the reliability of the final material.

If the parts of your aero engines and your planes are to have that reliability which the pluck of our flying men merits, the above considerations should be adequately recognised.

Conclusions.

It may be useful to summarise the views expressed in the foregoing paper on the manufacture and use of steel for aero work. Personally, my position is that of the investigator, and I have endeavoured, so far as space and time have allowed, to carefully weigh and indicate the different subjects and aspects which appear to me to be either deserving or in need of attention. I have endeavoured to avoid being dogmatic in any sense, recognising that any personal view can only be limited in its survey. At the same time I have not hesitated to state the impressions derived from my experience, and trust that the subsequent discussion and the views expressed will be similarly free from hesitation. It is only by a free exchange of ideas and unstinting collaboration between the different sections concerned that progress can be made. In the Introduction I emphasised the importance of using only the highest class of materials for aero work, and this, I think, will be uncontested. The necessity for scientific methods in works practice needs, I am sure, no further argument before this Society. As regards design and factors of safety, I have expressed the views occurring to a metallurgist in the hope that they might draw forth helpful criticism from the engineering side. And here let me once more make a plea for a still more perfect understanding between the engineering and the metallurgical branches; the work of the one is necessarily complementary to the other, and an unmistakable impetus to continuous evolution of practice will be induced in proportion to the degree of collaboration attained.

It may appear that I gave up too much space to the mechanical testing of steels, but in extenuation I would point out that it is my opinion, and, I think, the opinion of many others, we have much to learn not only with regard to the various methods employed in testing steels and material generally, but also with regard to the interpretation of such results. Incidentally, the continued correlation of the behaviour of different materials under the various forms of test cannot but give increasingly invaluable knowledge to the individuals who are concerned with questions of design.

The constitution and properties of typical steels have been dealt with and an attempt made to illustrate the manner in which they are likely to respond to the various forms of heat treatment. I hope that my remarks have sufficiently emphasised, although I do not think it was really overlooked, the necessity for a concentration on the heat treatment side of production.

Perhaps the most important point which I have endeavoured to push home is the prime necessity for efficiency in manufacture; in steel-making, in design, in forging, and in heat treatment, the efforts of the steel-maker, the stamper, and the engineer must be unrelenting. Not only does this apply to those in charge; more important still is it that the workman who ultimately carries out the work should be taught and encouraged to make his work a labour of love. It is the excellence of technique in the various items which is going to be the chief factor in making for reliability, and it is my hope that this small effort of mine will provide a little help to those at work in understanding something more about the steels upon which the science of aeronautics so much depends.

Some little time ago I visited one of the air stations, and was naturally professionally interested to hear the views of the pilots with regard to the reliability of the different items. One of the pilots replied to one of my queries by stating "The parts do not fail." His confidence in the materials of which his aeroplane was constructed was obvious. That, I take it, may be considered a compliment to the work of the entire industry, and long may the airman's confidence continue to be justified.

Before closing, I should like to thank Messrs. T. Firth and Sons, Ltd., and

Messrs. J. Brown and Co., Ltd., for the permission to here publish data derived from numerous investigations carried out on their behalf and for the use which I have been able to make of technical experience gained by them over a number of years commencing with the initial use of alloy steels for aeronautics. Particularly have I pleasure in thanking Mr. F. C. Fairholme and Mr. J. Wortley Fawcett for their helpful interest in the preparation of the paper. I should also like to record my appreciation of the assistance rendered by the members of the Research Laboratory Staff (particularly Messrs. Bolsover and Stanfield).

SOME METEOROLOGICAL CONDITIONS WHICH INCREASE THE DANGER OF FLYING.

BY CAPTAIN C. J. P. CAVE, R.E.

It may seem rather presumptuous for one who does not himself fly to discuss the dangers that may be met with in the air as though a landsman who had crossed the Channel a few times were to write on the navigation of a ship across the ocean. At the same time, it may be of some use to point out certain conditions of the atmosphere which seem to me to constitute dangers, although I may be mistaken in my estimate of some of these, and would welcome any information from pilots bearing on the subject. In fact, my paper is meant to elicit information rather than to give it.

At the same time I should like to protest very strongly against the idea that we have made so much progress in the science and art of aviation that we can afford to disregard the weather altogether, except perhaps in the case of fog. The idea is a common one, and has been often stated, but it seems to me that it is a most dangerous idea to foster. An airman cannot afford to disregard the weather any more than can a seaman. A seaman puts to sea in almost any weather, but the fact that storms take their toll of shipping is a proof that seamen cannot entirely disregard these things. Neither are airmen immune. Only last summer it was stated in our official communiqué that five of our aeroplanes had failed to return owing to a severe rainstorm.

Probably what is meant when it is stated that airmen can afford to disregard the weather is that so much progress has been made in the construction and design of aeroplanes that they can go up in almost any wind and can fly safely in winds that would have proved fatal to machines a few years ago. But there remain some conditions that are dangerous, and many that are severe hindrances to aeroplane work in war.

The chief conditions that suggest themselves to my mind as increasing the risks of flying are the following :—(1) Gales; (2) squalls; (3) bumps and eddies; (4) clouds; (5) rain, hail and snow; (6) fog; (7) lightning. It is possible that the number might be added to by experienced pilots, and it is also possible that some of the conditions that seem to me to be dangers may not really be such, but I suggest them in the hope of getting more information.

(1) *Gales.*—In the early days of flying strong winds were more formidable than to-day, but there are still occasions when the wind is so strong that machines are able to make little or no headway against it, and such a strong gale may arise with great suddenness and sometimes without much warning.

An example occurred on December 28th, 1914, when a small depression formed over the Bristol Channel and passed across the South of England, causing a gale that did a considerable amount of damage. In the South-East of England it was nearly calm before the onset of the gale, which sprung up with great suddenness. At Farnborough, for instance, an anemometer exposed 140ft. above the ground level registered a velocity of 80 miles per hour, when only a quarter of an hour previously it had been quite calm. (See Figs. 1, 2, and 3.) I do not

know that there were any aeroplanes flying when the gale began; probably not. The gale began in the afternoon in the South-West of England, but it did not reach the South-Eastern Counties nor the North-East of France till after dark. But a gale of 80 miles per hour 140ft. above the ground must have been

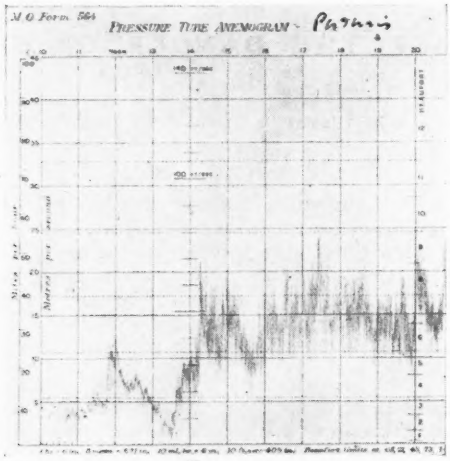


FIG. 1.
Anemogram, Falmouth, Dec. 28th, 1914.

considerably more at a height of 1,500ft., and I venture to think that if aeroplanes were flying when such a wind sprang up many would have failed to return to their aerodromes. If at the same time there were a development of low clouds when such a gale came on, aeroplanes would be likely to lose their bearings entirely.

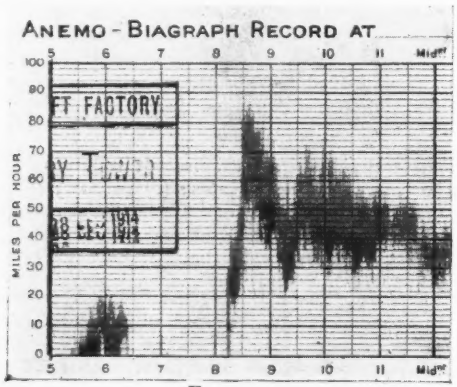


FIG. 2.
Anemogram, Farnborough, Dec. 28th, 1914.

Besides the loss of aeroplanes actually flying, a gale such as the one under discussion would cause severe damage to hangars and tents, and I believe that on this occasion considerable damage was caused in this way. Anyone who was in North-Eastern France at the time may remember the tiles and chimney pots with which the roads through villages were strewn.

Gales may spring up with great suddenness at all times of the year; in fact, I think that a summer gale may often give less warning of its approach than a winter one. Anyone who has done any sailing round our coasts must remember cases when they have been caught in gales a couple of hours or so after having been lying becalmed. I think that easterly gales in the summer are apt to spring up suddenly in this way.

A gale, however, usually gives warning of its approach, and may often be forecasted many hours in advance, and the weather map for the day will usually indicate when a gale is likely.

(2) *Squalls.*—A squall is a temporary rise in the wind above the mean velocity that precedes and follows it, the rise in velocity being continued over some minutes at least, and is thus distinguished from a gust, which only lasts a small part of a minute. Squalls are of innumerable degrees of severity. On a day of blustery north-west winds, when there are large cumulus clouds about,

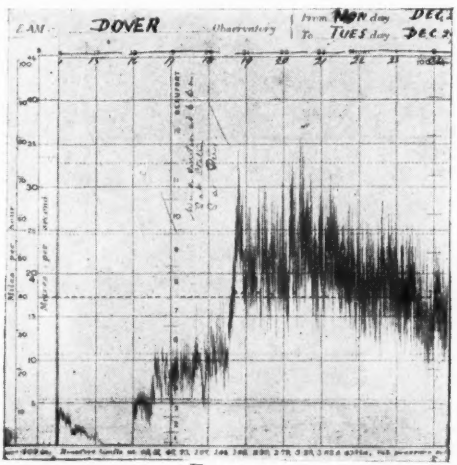


FIG. 3.
Anemogram, Dover, Dec. 28th, 1914.

one may have a succession of squalls, whose approach can be seen at sea some time before their onset. Such squalls are probably not of any particular danger to aeroplanes, as at sea they are not of much danger to shipping, except in the case of small open sailing boats, but in peace time at seaside resorts they take their toll of holiday makers who are sailing with the main sheet made fast.

More intense squalls are associated with thunderstorms, and they are all the more dangerous since they are often preceded by very light winds or even by a complete calm, and within a minute or so from their onset the wind is blowing at the rate of even 60 to 80 miles per hour. A typical example of such a squall occurred on August 2nd, 1906. As seen in the East of Hampshire, this storm came up from the direction of the Isle of Wight in the shape of a huge cumulus cloud with a great extension of false cirrus at the top, giving it the appearance of a giant mushroom; the day had been very hot and the air was very still. As the storm approached it was seen that heavy rain was falling, but there was no sign of wind to the untrained eye. A few minutes before the rain reached the observer a continuous roar was heard, and as the first drops fell a furious blast of wind arose; the wind only lasted a few minutes, and in three-quarters of an hour the storm had passed and the weather was fine again. The storm passed

over the South Downs, and the same storm or another moving parallel to it reached Guildford, where the damage done by the wind was very great. I cannot imagine that an aeroplane caught in such a squall would not have been in danger. Certainly an airship would have been in the very greatest danger and could hardly have weathered the storm.

The squall in front of an ordinary thunderstorm is probably a modification of another variety known as the line squall. The sequence of events in a line squall is somewhat as follows:—A bank of clouds is seen extending along the horizon, the upper parts being white and in shape like ordinary cumulus, though the whole cloud usually appears of a more uniform height, and not broken up into such distinct peaks as is ordinary cumulus. (Fig. 4.) As the cloud approaches it is seen to be extremely dark below, and it usually extends in a long line, stretching from horizon to horizon, but owing to the effect of perspective it

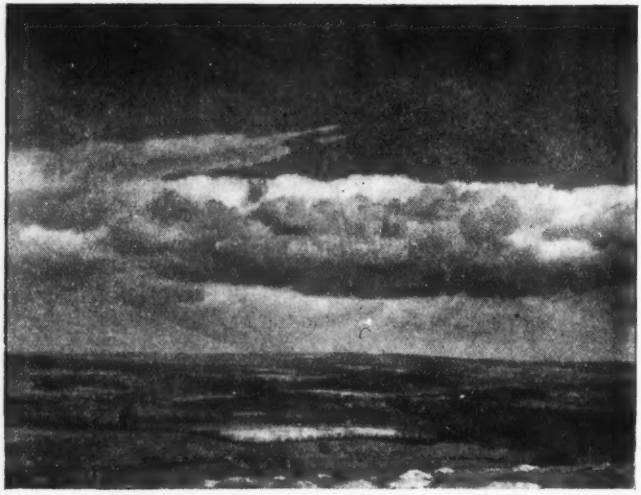


FIG. 4.
Part of a squall cloud; the cloud stretched right and left as far as could be seen. When it came overhead there was a moderate squall with snow.

appears like an arch in the sky, the summit of the arch coming nearer and nearer overhead. As the cloud reaches the observer a violent squall springs up, the wind veers rapidly or even suddenly, rain falls in torrents and is often accompanied by hail, and there may be thunder and lightning; at the same time the temperature falls considerably, a fall of five or ten degrees being common, and it is sometimes as much as 20 degrees. When the cloud is approaching and is nearly overhead a curious sinuous line is seen at its base extending right along the front of the cloud, and it is this line which gives the name of line squall to the disturbance. After the first blast the wind blows strongly for a time and the heavy rain lasts for half an hour, more or less; this is followed by a less intense fall of rain and by decreasing wind, and after an hour or so the weather often clears up and becomes fine.

A line squall is only a few miles across, but it may be several hundred miles long, and it advances across the country broadside on at the rate of 20 to 40 miles per hour; one such squall has been traced from Cape Wrath to the centre of France, another from the North-West of Ireland to the centre of Germany. The

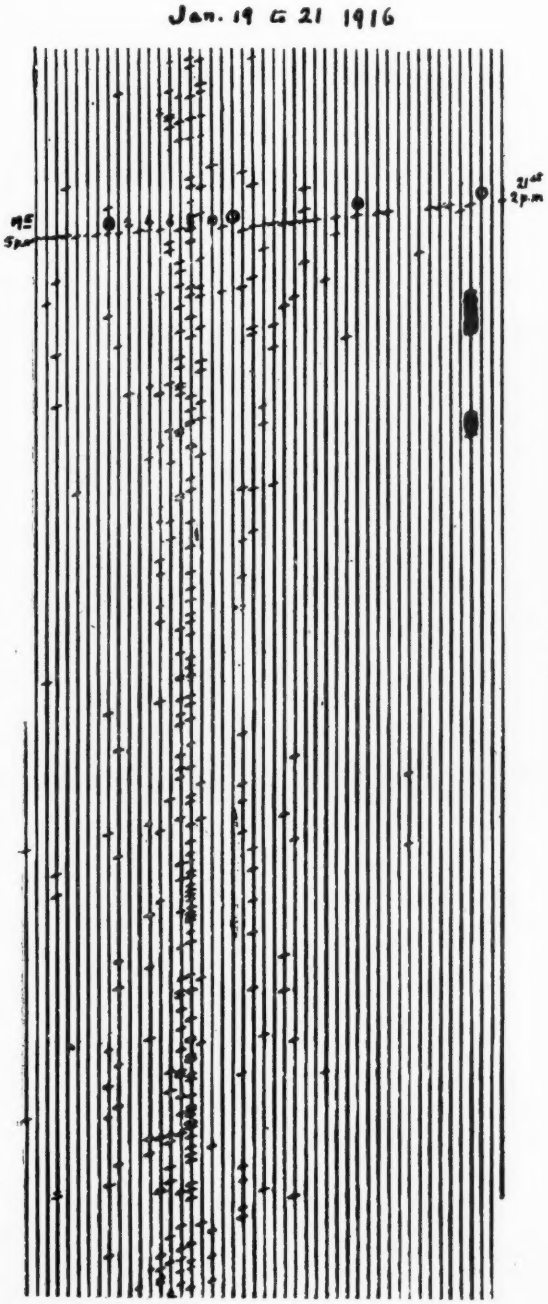


FIG. 5.

Trace of Automatic Lightning Recorder, Jan. 19th to 21st, 1916, South Farnborough. The cross strokes show the times at which flashes of lightning occurred within range of the instrument. The range was probably about 250 miles. The line of strokes near the top are time marks made every hour.
• is midnight, O midday.

list of disasters caused by line squalls is a long one; the best known case is that of H.M.S. "Eurydice," a training ship homeward bound that was struck by such a squall when off the Isle of Wight on March 24th, 1878, and foundered with heavy loss of life.

Besides the blast of wind in front of the squall, there are great up currents in front and down currents near the middle of the squall, with much eddy motion between them. Such conditions could hardly fail to be dangerous, and though an aeroplane might possibly come safely through them, it is hardly likely that an airship would.

The onset of a line squall is generally sudden, though anyone with a very little training in meteorology can see it coming while it is still some way off. On January 20th, 1916, a line squall passed across the country from north-west to

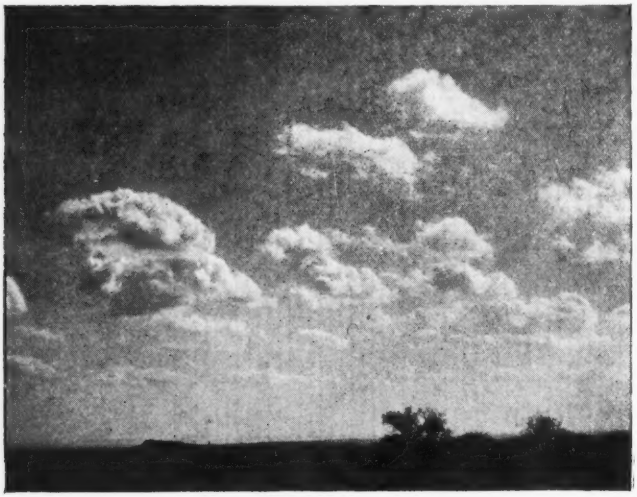


FIG. 6.
Very small cumulus clouds beginning to form.

south-east, reaching Farnborough at 10.30 a.m. The morning had been fine, and a number of machines were out on the Common; the squall came on suddenly, and several machines were damaged before they could be got back into their sheds. A storm such as this one can be predicted with some success if the machinery is ready for the purpose. The general conditions favourable for line squalls can usually be forecasted from the Daily Weather Map prepared at the Meteorological Office, but unless a line squall occurred at one of the Meteorological Office Observing Stations at the time of the taking of the meteorological observations or shortly before, the existence of the squall may not be noticed on the map. The squall of January 20th, 1916, was first observed in the South of Ireland at about 4.30 a.m. It was accompanied by much thunder and lightning. It crossed the Irish Sea and reached the coasts of Cornwall and Wales at about 7.30 a.m., and moved across the country in a line 100 miles long or so, the movement being, as usual, at right angles to its length. Now this storm did not affect any of the Meteorological Office Observing Stations, and hence its existence was not known officially, as one may express it. But since a squall of this type is perfectly easily recognised, its coming might have been foretold if the proper machinery had been in existence to deal with it; such a warning might have been

received in plenty of time for the aeroplanes on the Common at Farnborough to have been put in their sheds in safety.

That there is time for such warnings to be given is shown by the fact that recently I had a telegram from Upavon telling me that a line squall had just passed over; the telegram was received about ten minutes before the squall reached Farnborough. Ten minutes is doubtless too short a time for the warning to be acted upon, but a station further west than Upavon could have sent a warning that would have been received in plenty of time.

It appears to me that it would be quite feasible for an observer at every aerodrome to send a telegram to some central office when a line squall took place; at the central office the general weather conditions would be well known, and therefore the direction of motion of the squall could be foretold. In addition, other reports would come in as the squall reached other aerodromes, and the rate

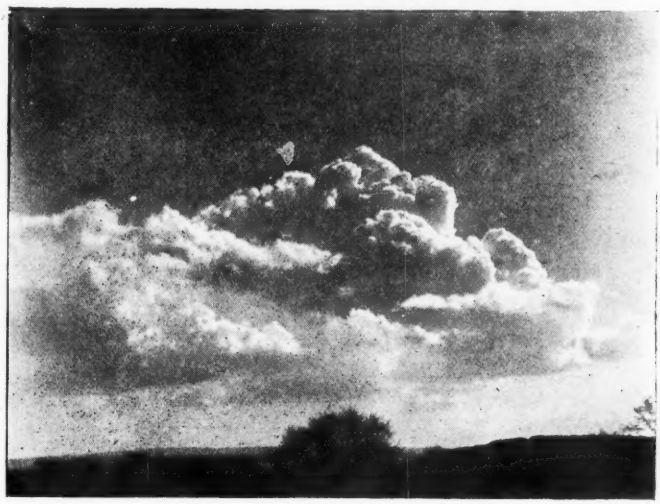


FIG. 7.
Simple cumulus with hard edged rounded top.

of travel could be obtained with some accuracy. Warning could then be sent to all aerodromes which were likely to be affected, and some signal might be hoisted in a conspicuous place where it could be seen not only by those who were responsible for machines on the ground, but also by pilots who were flying in the immediate neighbourhood of the aerodrome. Some such organisation would not be complicated, and would only occasionally have to be put into use, but it might be the means of saving machines, and possibly lives also. If the information were to be sent out by wireless the warnings would be received still earlier. But weather forecasts are looked on in some quarters as the subjects for jokes, and it will take some serious accidents caused by line squalls before anything practical is likely to be done. A severe line squall is generally accompanied by thunder and lightning, and an automatic lightning recorder would indicate its approach, especially in winter, when thunderstorms are almost entirely of the line squall type. The lightning recorder at Farnborough began to record lightning at about 5 a.m. on January 20th, 1916, and it was quite evident from the chart that something quite out of the ordinary, for the winter, was occurring. (See Fig. 5.)

(3) *Bumps and Eddies.*—I do not propose to deal with these at any length. The danger from bumps is small with modern aeroplanes, though in the early days of flying they were a source of great danger. Pilots are far better qualified to speak of bumps than is a meteorologist who has only flown a few times as a passenger. Bumps are mostly due to rising currents of air over surfaces of the ground that are at different temperatures and to eddy motion due to the wind blowing over irregularities of the surface. They also seem to occur at the cloud layer when there is a sheet of cloud, and they occur, of course, with cumulus clouds.

In this connection there is a point on which pilots could give some information. At Farnborough an easterly wind is far more bumpy than a westerly wind. Is this due to some local configuration of the ground, or is it something inherent in an easterly wind? I suspect that there is some connection between bumpiness and easterly winds, but what it is I cannot attempt to explain. An easterly wind

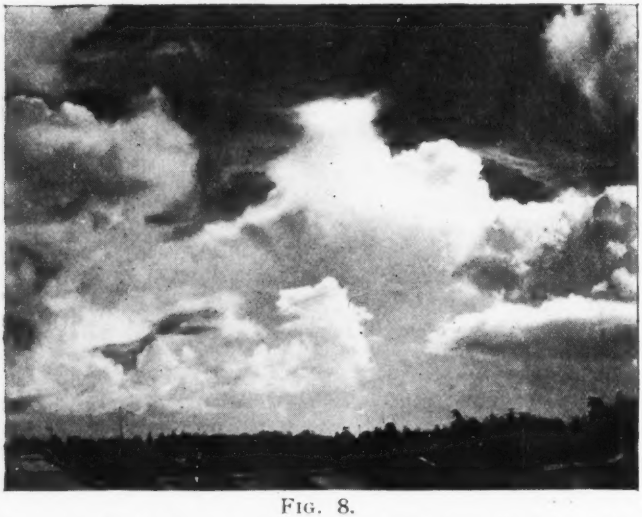


FIG. 8.
Cumulus beginning to turn into cumulo nimbus; the top of the cloud in the centre is becoming soft edged and fibrous.

has, seemingly, peculiarities of its own; it is said, for instance, that there is always more sea in the Channel with an easterly wind than with a wind of corresponding strength from other quarters.

(4) *Clouds.*—Clouds may be a danger in several ways. In the case of cumulo nimbus clouds the heavy rain, and possibly hail, or the snow in winter, may prove extremely dangerous. Such clouds, too, are often of great extent, and are the seat of very rapidly ascending currents of air. A pilot might have to fly a considerable distance before getting clear, and might easily lose his bearings. A cumulo nimbus cloud is one that should be avoided if it is possible to do so, and as these clouds often occur in isolated masses, it may be at times possible to fly round them. A cumulo nimbus, the true shower cloud, from which rain, hail, or snow is falling, may be distinguished from simple cumulus by the fact that the top of the former cloud, instead of being rounded and hard-edged, is brushed out into a soft-looking mass of fibrous cloud called false cirrus. (Figs. 6, 7, 8, 9.) It is true that showers fall from simple cumulus clouds, but the really heavy falls are from cumulo nimbus.

Cumulus clouds are more common in summer than in winter. They usually begin to form in the morning as the day gets warm, and reach their greatest development about two or three in the afternoon, after which they generally begin to disappear, and by sunset or soon after the sky may be quite clear even after a day of great development of this form of cloud. But they may be met with at other times, and they form after sunset in the summer when shallow depressions, bringing thunderstorms, are approaching.

Low sheets of cloud may prove a hindrance to work with aeroplanes, and if they are very low they may cause difficulties for a pilot in finding his way back to the aerodrome, or even difficulties in landing at all. The possibility of low sheets of cloud forming after a clear morning, especially in winter during unsettled weather, should always be borne in mind. A glance at the latest weather map will often be a valuable guide as to whether a fine morning is likely to last.

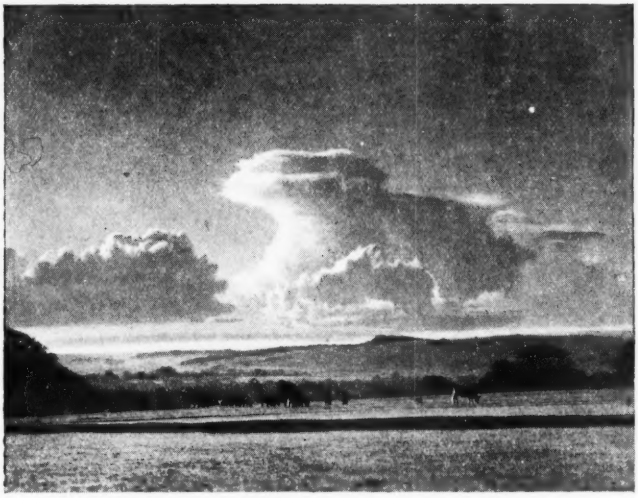


FIG. 9.

Fully developed cumulo nimbus; the whole top of the main cloud is a mass of false cirrus. Note the difference between the top of the main cloud and that of the small cumulus on the left.

Take, for instance, the map for 7 a.m. on December 24th last. (See Fig. 10.) This shows that the sky was clear in North-Eastern France, but subsequent reports show that it became overcast and rainy, and that there was a great extension of low cloud. Anyone who had in the least followed the weather on the days preceding this date and who had seen the map for the day, or even for the preceding evening, would have realised that the fine morning was not likely to last beyond a very short time. I maintain that aeroplanes going up on such a morning would be extremely likely to find low clouds extending to within a few hundred feet of the ground before their return, and they would therefore be in some danger. It might be that it would be necessary to incur the danger for the results that might be obtained, but it would be absurd to say that such meteorological conditions would not add to the risks of flying.

(5) *Rain, Hail, and Snow.*—The danger arising from these is obvious, and it varies, of course, from nothing in the case of very light falls to a real danger in the case of very heavy falls. Danger from very heavy rain or from hail can

often be avoided by keeping away from cumulo nimbus clouds. Such clouds are often seen in isolated masses, some miles in circumference perhaps, but still they can on such occasions be avoided. In the case of a line squall it is, however, not possible to fly round the cloud, for this often extends in a long line for several hundred miles. It might be possible to fly over a line squall and so avoid the rain, hail, and turbulent motion of the air associated with the disturbance, but I do not know that there is any evidence as to the heights to which the line squall disturbance extends; it probably varies on different occasions.

Hailstones in summer thunderstorms constitute a real danger to an aeroplane that might be involved in the storm, for they sometimes attain an enormous size. In the British Rainfall Organisation's Volume for 1913 is a photograph of hailstones that fell in Essex on May 27th of that year. Although they had partially melted before they were photographed, they are still shown as nearly as large as a hen's egg that was photographed with them for comparison. (Fig. 11.) They fell at Wickham St. Pauls, near Halsted, in Essex. At Great Yeldham there was a fall of similar hailstones. At the latter place the fall lasted

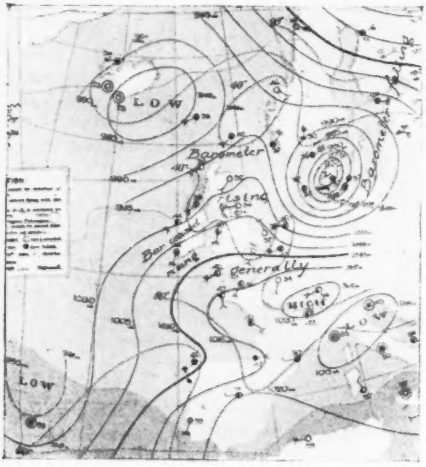


FIG. 10.
Weather Map for 7 a.m., Dec. 24th, 1916. (From the Daily Weather Report of the Meteorological Office.)

only eight minutes, but "the devastation was great," an observer says. "Crops were smitten to the ground, glasshouses all smashed, tarred roof felting cut to ribbons, corrugated iron riddled, tiles and windows smashed in thousands. . . . Man and beast were bruised, and among other animals killed on my own farm I saw rooks, wood pigeons, full-grown hares, partridges, pheasants, rabbits, and various small birds, wild ducks, farmyard fowls, and three cygnets." At Haverhill the stones varied from the size of nutmegs to that of walnuts. At Harston the stones were 1¼in. in diameter, and at Sheerness ⅝in. to ¾in. Damage from hail was reported from a wide region in the Eastern Counties on this day.

Doubtless it will be said that this was an exceptional occasion, and no doubt it was, but a glance through one of the Rainfall Volumes shows that very heavy falls of hail occur in the summer months in all parts of England. For instance, besides the 24th of the month, heavy falls occurred in May, 1913, on several occasions; on the 10th very heavy hail fell at Bolerno, in Midlothian; on the 26th hailstones over 1in. in diameter fell at Bulvan, and at Bishops Castle scores

of windows were shattered; on the 30th at Gravesend the hailstones were 1½in. in diameter; on the 31st at Waltham, on the Wolds, there was an exceptionally heavy fall of hail.

A glance through the records for any summer month shows that so-called exceptional falls of hail are fairly common, and it scarcely needs pointing out that hailstones far smaller than hen's eggs would have fatal effects on an aeroplane that met them.

(6) *Fog.*—Perhaps fog is one of the worst of the dangers that beset flying, and I should like especially to call attention to fog to those who maintain that at the present time aviators can afford to disregard the weather. The subject of fog, however, has lately been dealt with before this Society by Major Taylor. I may, however, give an example of how a fog may be formed by the mixing of air at different temperatures. On April 3rd of this year there was a shower of snow at Farnborough which was followed immediately by bright sunshine; after

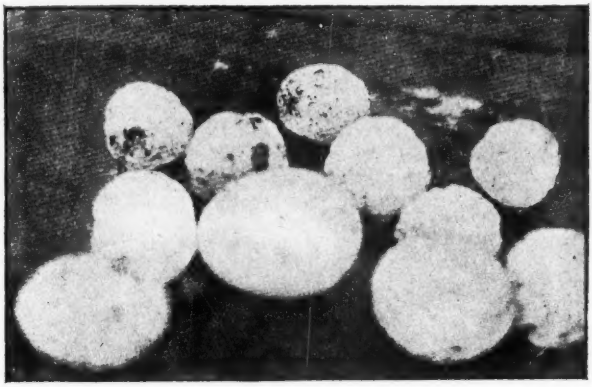


FIG. 11.

Hailstones which fell at Wickham St. Paul's, May 27th, 1913.
(From " British Rainfall " for 1913.)

a few minutes a mist began to form over the aerodrome, and for a short time wreaths of fog covered the Common. (Fig. 11.) No doubt the sun heated the ground and warmed the air in contact with it; the relatively warm air, which would have been fully saturated, was mixed with the cold layer just above—air that had been cooled by the melting snow before the sun came out. The saturated warm air was chilled and its moisture was condensed into fog.

(7) *Lightning.*—It is difficult to say what is the danger to be apprehended from lightning as such. The dangers to flying from thunderstorms are due to the squalls and to the heavy rain and hail that accompany them. It is possible that the actual danger from lightning to an aeroplane flying through a thunderstorm may be no more than that incurred by a pedestrian walking across an open common during a storm. A pilot who was flying above a thundercloud last summer reported that long sparks were given off by his machine at intervals. It is very likely that this happened every time there was a flash of lightning from the cloud below him. But no inconvenience was caused by the sparks. In the case of an airship, however, it would be far otherwise, for quite small sparks might ignite the hydrogen. Lightning is also a danger to kite balloons owing to the conducting wire. There are several cases on record in which meteorological kites have been struck by lightning, and as some of these occurred when

there was no thunderstorm in progress, it must be remembered that clouds may be highly charged with electricity at times when no actual storm is going on.

There is a particular type of violent thunderstorm in which most of the lightning takes place from cloud to cloud, and when it is almost incessant. It would probably be dangerous for an aeroplane to enter such a cloud, but the appearance of the danger is so obvious that it scarcely needs pointing out.

I hope that what I have said will make it clear that the airman is not entirely immune from the disturbances in the medium in which he flies. If I could persuade aviators in general to take meteorology rather more seriously I should feel that I had not read this paper in vain, but I am afraid that weather maps and forecasts are looked on by many, though not by all, I am glad to think, as mere matters of guesswork, and not worthy of serious consideration. The

FIG. 12.
Fog forming on the aerodrome at Farnborough when a snow shower was followed by bright sunshine.

public in general exhibit a lamentable ignorance of the very elements of meteorology, which is largely due to our educational methods. I do not wish everyone to become a meteorologist, but there is no reason why everyone should not take an intelligent interest in the movements of depressions and anticyclones, and have some faint knowledge of what these terms mean. The English are supposed to talk so much about the weather that it is a pity they should not know what they are talking about, and those who are responsible for the safety of aeroplanes and airships ought to know as much about the weather as, say, a master mariner in the mercantile marine. There are cases where ignorance may be criminal.

AIRCRAFT AND MOTOR CAR ENGINE DESIGN: CONTRASTED FROM THE STANDPOINT OF A DESIGNER AND MANUFACTURER OF BOTH TYPES.

BY LOUIS COATALEN.

In addressing myself to the members of the Aeronautical Society of Great Britain, the oldest institution of its kind in the world, I have to bear in mind that a section only of that membership is composed of designers of internal combustion engines. Therefore I have striven to make my remarks this evening as intelligible as possible to those who have not had such training.

At the outset, too, I take the opportunity to thank the Admiralty for giving permission for a paper to be read on so instructive a subject with a view to its being discussed within the limits which it is necessary to impose on a topic of this sort in face of our being engaged in the greatest war in history.

That belief which appears to obtain in some quarters to the effect that the design and production of an aircraft engine is akin to that of a motor-car one proves, on even casual investigation, to be what the old writers would have styled a vulgar error. By taking a few points which come most obviously to mind, we discover at the very outset that the problems involved by the two propositions are fundamentally different.

Chief Characteristics of a Car Engine.

Consider for a moment the chief characteristics of an internal combustion engine for motor-car service :—

(1) Weight is practically no object.

(2) Cost is of the utmost importance, therefore there must be the minimum of machining, as instance the fact that the connecting rods of a motor-car engine are not milled, nor are the crankshafts machined all over.

(3) It must be capable of production in great quantities at minimum cost; otherwise, with the least amount of labour.

(4) It must be silent to the extreme of what is practicable.

(5) The maximum effort of which the engine is capable is not needed to be maintained for long at a spell. It seldom works at full power, and the brief duration of such effort explains the extraordinary reliability of even the inferior types of car engines.

(6) Flexibility, giving a constant torque at a crankshaft speed from 300 revolutions a minute to 2,000 revolutions a minute.

(7) Of course, this is a torque which corresponds to a very low mean effective pressure, namely, 80lbs.

(8) The compression is relatively low and the valve area small, the cam forms being easy and the valve springs light.

(9) The system of lubrication, wherein the oil is carried in the base-chamber of the crank case, suffices.

(10) The maximum horse-power required to be developed by any one engine rarely approaches 100. In the vast majority of cases it does not exceed 30.

And One for Aircraft Service.

By contrast, the factors governing the design of an aircraft engine may be enumerated thus :—

(1) Weight is of prime importance.

(2) Cost is not the deciding factor provided the necessary amount of power

is obtained for the given overall dimensions of the engine, for its weight both as regards material and fuel, water and lubricant consumption; and that the desired degree of reliability is obtained.

(3) The amount of labour necessary to produce a satisfactory aircraft engine of high output is, and will be, always many times what is necessary in the case of a car engine, and is a matter of secondary importance provided the desired results are obtained. No machining is too expensive if it saves weight.

(4) Silence is relatively unimportant.

(5) The aircraft engine does all its work at practically full power.

(6) Flexibility, or evenness of torque, is of very secondary importance, because an aircraft engine is required to develop maximum torque at practically one speed only, or, at most, at an extremely narrow range of speed.

(7) But at its working range of speeds a very high brake mean effective pressure, say 130lbs., is called for.

(8) The compression must be relatively high and the valve area large, while the valve springs must be stronger than for a motor-car, due to the cam form imposed.

(9) The high mean effective pressure necessary, coupled with the fact of the engine doing nearly all its work at full power, involves a completely different point of departure in determining details of design and, notably, the exploitation of new methods of achieving lubrication. Experience has demonstrated abundantly that when the base-chamber is used as an oil well, as in motor-car practice, the lubricant soon becomes too hot, therefore too fluid, resulting in reduction of pressure to the main bearings; hence the evolution of the dry sump system for lubricating aircraft engines. Oil viscosity varies greatly with the temperature.

Evolution of the Dry Sump Lubricating System.

I would mention in passing that the history of the engine dry base lubricating system is neither more or less than the story of my racing experience on the Brooklands track. In the course of long runs on it years ago it was found that the oil pressure went down more and more the longer each run was continued. Naturally we tried one brand of oil after another with the view to discovering which would retain its viscosity most effectively. Of course, castor oil gave greatly superior results to mineral oils. Even so, however, it soon became plain that the problem was one that could not be solved entirely by the use of a vegetable oil. Indeed, results were quite unsatisfactory, notwithstanding that we greatly increased the effectiveness of the pump employed. Therefore my next step was to use the same pump to force the oil out of the base-chamber through two 1in. copper pipes arranged round the car. We returned the oil direct from that process of cooling to the service of the bearings under pressure. This proved a great advance as regards maintaining pressure; but the scheme involved all the inconvenience of a long circuit for the oil in connection with which all the cooling was achieved under pressure, because the oil passed quite round the car before being returned to the bearings.

Therefore the next stage was to employ two pumps. One forced the oil out of the base-chamber through a cooler, from which it passed into a tank placed at the back of the car. In this tank the oil was not under pressure of the pump, for the tank itself was merely under atmospheric pressure. In practice it was found that this was really a notable improvement. Thus the bulk of the oil was kept all the time in the tank, which itself was in a draught while the car was travelling, while the base-chamber itself was kept quite empty. From the tank the oil passed to the pump, and was so forced by it into the bearings. Therefore the oil was under pressure only for a short distance, namely, from the pump to the bearings, because, as has been explained already, the tank itself was under atmospheric pressure.

Only when we had arrived at this stage was it found that racing cars with

engines of high output could be run for more or less indefinite periods without the temperature of the lubricating oil attaining more than 66 degrees Centigrade, at which warmth a very good working viscosity was retained.

(10) Lastly, in contrasting the standard car engine, the racing car engine, and the aircraft engine propositions under the headings that have been selected for the sake of illustration, it is to note that the total amount of horse-power required to be developed by practically all aircraft engines to-day is about 100 minimum, while the maximum totals several hundred horse-power per unit.

Two Distinct Propositions.

It will be seen from those ten points of contrast which, doubtless, might be increased in number, that the aircraft engine of to-day is not akin to the standard motor-car one. Admittedly, the twain are collaterals, both deriving from a common stock, the four-stroke cycle, petrol internal combustion engine. For the rest, the aircraft engine of to-day is, perhaps, as little like the standard motor-car one as that resembles the variety used on a commercial motor vehicle or that installed in a motor-boat. In fine, it may be said that, as the stationary gas engine resembles the portable petrol variety to that meagre degree, and scarcely more, does the motor-car engine resemble the aircraft type. It cannot be proved that the aircraft engine has been developed from the touring car variety. On the contrary, it can be demonstrated abundantly that the aircraft engine is quite a distinct branch of the development of the internal combustion engine. Hence many firms that have been strikingly successful in producing car engines for either touring or commercial use have experienced great and, in some cases, unsurmounted difficulty when called upon to change over to the manufacture of power plant for aircraft. The differences apparent in the design become even more pronounced when they are translated into manufacturing problems in the shops.

On the other hand, we may not lose sight of the likelihood that the very rapid evolution of the aircraft engine during this war, and the extraordinary manufacturing experience and developments of which that is the outcome, will at some future time exercise a more or less temporary effect on the design and manufacture of engines for car service.

Be this as may be, in broad terms I am of opinion that the two schools of design, one concerned with each of these problems, will continue to advance for the most part along two distinct lines which will rather become more than less divergent. Hence on the present occasion little further attention need be devoted to standard engine design for car practice. Suffice it to observe that to date the non-technical opinion of the buying public, which opinion is not to be depreciated altogether, has exercised a not inconsiderable and, on occasion, detrimental influence on the designer and manufacturer. It will be observed, incidentally, that the element affects the proposition of aircraft engine design scarcely at all, especially under the conditions which are beginning to govern the industry towards the conclusion of the third year of war.

The Analogy of the Racing Car Engine.

By contrast, there is another type of engine specially built, as distinct from standardised, and which is fitted to a few motor chassis only each year in relation to the total number produced, because it is evolved and employed for racing purposes solely. Admittedly, in the beginnings of the motor industry the racing car of one year became the standard vehicle the succeeding season. With the lapse of time, however, racing became so highly specialised that if the individual competitor was to enjoy any prospect of success during the last four or five years the racing engine had become a proposition utterly distinct from those standardised for service or ordinary civilian motor vehicle uses. This point is proved by a summary of the main characteristics required of a racing car engine,

and which we find are to a considerable extent identical with those needed for an aviation engine. Thus :—

(1) Weight is of importance.

(2) Cost is unimportant.

(3) The amount of labour and the time necessary for production are matters of relative indifference provided the maximum output of horse-power is obtained for a given size of engine. That demand has led manufacturers to employ overhead valves, which are also used in aviation service and which so far have been employed comparatively little in standard car practice, partly on account of the principle not being so quiet in operation as the side-valve system. Every part of a racing car engine must be machined. The connecting rods are milled to the minimum section, and so forth.

(4) Silence is of no importance whatever.

(5) The racing car engine does all its work at practically full power, but the evenness of its torque has to be extended over very much wider ranges of speed than is needed so far in the case of an aircraft engine. From 1,600 to 3,400 crankshaft revolutions a minute is called for in the former case, whereas in the latter the normal speed is 2,100. The last-named figure chances to be no less than 1,300 revolutions a minute slower than the capacity of Sunbeam racing car engines. Therefore it will be appreciated that the engine for racing car service is submitted to bigger stresses than the present-day aviation engine; but that this period of high stress in the case of the vehicle variety is much shorter than obtains in that of the aviation type unless, indeed, the car is being run on a track. Even in that event twelve consecutive hours is considered a very long spell, whereas in aircraft service that period of uninterrupted power output is held to be all in the day's work.

(6) Under the heading of flexibility the engine for your racing car must be more akin to standard car requirements than to those of aircraft service. This characteristic, therefore, works out as a disadvantage to the racing car engine. When employed on dry roads with efficient gears and so forth the starting torque mounts up to a high figure, whereas in the aircraft engine at starting there is no load on the propeller. It increases, roughly, with the cube of the revolutions.

(7) The racing car engine resembles the aviation type in that a very high mean effective pressure has to be obtained with both. In some racing car engines it has amounted to 135lbs. to the square inch, taken from the brake horse-power developed at the flywheel.

(8) As the problem is power for engine weight and volume, and not silence and low cost, great freedom is allowed the racing car engine designer as regards piston clearances, valve timing, compression, largeness of valve area, strength of valve springs, and so forth, the opportunities in this connection approximating much more to aviation than to standard car engine practice.

(9) The high mean effective pressure necessary, coupled with the fact of nearly all the work being done at full power, calls for lubrication methods quite distinct from standard car practice, albeit as yet these have rarely approximated to that of aircraft engine practice, though the problems of maintaining pressure in the oil circuit and of keeping the temperature of the lubricant normal are common to racing and aviation engine service.

(10) Comparatively large horse-power is needed in the case of all engines for racing cars, the average being anything from 80 to 225 horse-power, therefore much more on a plane with the demands for aircraft service than with those for the touring car, the town carriage, or the utility motor vehicle.

Lastly, outside influence, traceable in the cases of designing the private car engine and the commercial motor vehicle one, is scarcely, if at all, to be detected in those of the racing car engine and of the aircraft variety. The racing car type

has been developed with almost amazing rapidity through various stages along the lines of maximum power combined with low, as distinct from minimum, weight, and with the utmost reliability, notably with a view to enabling the machine to be run for long spells without loss of power.

We might, perhaps, complete our survey of the inter-relationship of these three branches of motor engineering enterprise by adding that in the aircraft engine we have to economise weight considerably over the degree that will suffice for racing car practice, while economy of fuel and oil consumption are also more important in the case of the former than of the latter. Admittedly, in the racing car engine those two features constitute a special and important factor, but not one that has had to be studied yet on entirely different lines to standard car practice. Accordingly, perhaps we might conclude not unreasonably by stating in general terms that one stage in the development of the aircraft engine is represented by racing car enterprise as well as, perhaps, by certain sporting motor-boat engine work. Endeavours in these directions provided us with the data from which were designed the first engines evolved on lines to be of such efficiency as the present-day aircraft variety.

In face of our being now in the third year of war, and therefore for the most part somewhat out of practice in the matter of racing car engine design, whereas the leading firms in the industry in Europe have by now accumulated much experience of standardising aircraft engines, though of recent years none of them have ever standardised any for racing service, it may be said that the data on which aircraft engines are being designed to-day derives wholly from cumulative experience of aircraft engines, and has ceased to depend in any way on racing car experience.

Indeed, on the coming of peace, doubtless it will be found that the position has been wholly reversed from that which obtained before the war. In the future not a little of racing car engine design may derive from aircraft engine practice.

Desiderata in Aircraft Engine Design.

To approach the problem from the correct point of view, we must recognise that the outstanding desiderata in designing aircraft engines to-day may be summarised thus :—

(*a*) Light weight, combined with low fuel and oil consumption, per horse-power.

(*b*) Reliability.

If we can but attain those characteristics with units of not less than, say, 200 horse-power—better still, if we can exploit them in units each up to 600 horse-power—then we can afford more or less to neglect other desiderata as being of minor importance. Nevertheless, happily we can already go a far way towards realising what we might style the minor desiderata, which at this period of the war include :—

(*a*) Simplification to the utmost in face of these engines being placed, for the most part, in the hands of a great number of men semi-skilled in even flying and maintaining them.

(*b*) Foolproof as much as possible in that some of the most daring Service fliers have not either the temperament or the understanding to spare the engines of which they are put in charge.

(*c*) Accessibility in face of the frequent attention needed by all aircraft engines and of the fortunes of war rendering it necessary on occasion to replace the most vital parts.

(*d*) Standardisation because for the first time in the story of motor engineering we are making engines of high output in series in place of about a half-dozen examples at a time.

(*e*) Suitability of exterior form that the power plant may be accommodated conveniently in the aircraft and occasion the minimum displacement.

Influences on the Aircraft Constructor of the Particular Employment.

Thus there are strict limits to the diametrical size of radial engines, whether of the rotary or of the stationary type, which it is profitable to employ for aircraft work; while in regard to the vertical, or to the V-type engines, the nature of the particular service to which each individual engine is to be put likewise imposes certain limits. In certain cases strict limits must be set to overall length of the engine, particularly at a time of war in the air, when, at need, it is essential to lose the minimum time in altering the flight path of the machine from a diving attitude to a very steep climbing one. Again, some sorts of aircraft call for the minimum engine head resistance, but are less imperative as to overall length; hence the six-cylinder type would be suitable for such service, whereas the V-type variety would not be.

Uniformity of Certain Characteristics.

In other words, at this period it is impossible to lay down any arbitrary rules as to any one type of aircraft engine being suitable for the needs of all aircraft service. Those needs are almost as various as are the demands for special varieties of steel and of alloys. Moreover, they are likely to multiply with the lapse of time. Yet between the widest varieties we perceive the essential characteristics of demand to be uniform. This is a great gain alike to the designer and to the manufacturer on the one hand, and to the Services on the other. It means that, when the right scheme of design is evolved, the least possible disturbance is caused in the given factory, though it be concerned with producing power plant of various size, weight, and horse-power output. Therefore the maximum production can be attained, while the problems of management and repair are correspondingly reduced to the minimum. Interchangeability can be exploited to the maximum, and, once the mechanics and pilots have mastered the principles of whatever system of construction is in question, it is found that those principles are applicable to all varieties of the given system of construction.

Demand in a State of Flux.

When we come to systems of construction, again there can be no laying down of hard-and-fast rules, for the sufficient reason that the suitability or otherwise of systems are predetermined by the demands of the aircraft constructor and the aircraft user, which demands are ever varying. Therefore what system may be the most suitable thing possible to attain satisfaction of the demand of to-day is not necessarily the principle on which to work for satisfying the demand of next year, or of five years hence. Thus, while we may make bold to criticise the suitability of this system or that to satisfy the insistent demand of the hour, we must have a care not to be drawn into the making of sweeping assertions about the practicability of any system of construction for the necessarily nebulous needs of the future.

Difference Between Might Be and Must.

Probably it is little realised that if the aircraft engine designer had not to think of the means at present available to the manufacturer, nor of the time factor, nor of those to whose tender mercies the standardised product will be submitted, he would produce very different designs to fulfil any given purpose from those he evolves to-day.

Aircraft engine design resembles motor-car engine production in this particular, that it is all the time a question of compromise. The most successful designer is he who exercises the soundest judgment in weighing a hundred and one factors of the hour and who gives the shrewdest estimate of the relative value of each.

Having thus striven to give a notion of what one might style the psychology of aircraft engine designing at this or any period, let us take some of the

governing factors of to-day somewhat more in detail. It is not deemed desirable, therefore it is not proposed, to give in a paper of this sort particulars of any engines such as are being or are about to be used by the Services in this war. Instead, it is held to be preferable to review with more or less detail the points that have received most attention in the development of the design of the latter-day aircraft engine.

The Three Main Schemes.

In regard to the general arrangement of aircraft engines, there are several main types, each of which involves advantages as well as disadvantages. The business of the designer is to effect the best compromise possible to fulfil the particular class of service that is had in mind in scheming the individual engine.

Of course, multi-cylinders are common to all types of aircraft engines. But the arrangement of the cylinder groupings and settings differs entirely as between one type and another. Doubtless the most generally favoured form is the V type with either 12 or eight cylinders per unit, these being set in two rows on a common crankcase whereby one crankshaft suffices because one crankpin serves for each pair of opposed cylinders.

Undoubtedly next in order of importance is the radial type, in which the cylinders are set in one or more planes with axes radiating from the centre line of the crankshaft.

The two sub-divisions of the radial type of engine are the rotating and the fixed variety.

What we may style the straight-line engine constitutes the third main type. In this four, more generally six, and, in a few examples, even eight cylinders, and twelve, are placed in a line and are set vertically on a crankcase, the pistons and connecting rods acting on a crankshaft with one crankpin per cylinder in the orthodox fashion of motor-car engine practice.

The Sphere of Utility of Each Type.

Inasmuch as each of these three types has advantages peculiar to itself, it follows that each is the most suitable so far available for some particular form of aircraft. For instance, the cross-section or wind resistance area per horse-power is least in the straight-line engine and most in the rotating radial type. This includes the loss of power necessary to rotate the engine. The fore and aft length of the engine, however, which is of great importance in some aircraft, is least per horse-power in the case of the rotating radial type and greatest per horse-power in the straight-line engine. Moreover, when the straight-line engine is water-cooled, as is generally the case, the rotating radial type gains a further advantage on the score of decreased weight per horse-power. Against this, however, the economy of fuel and oil consumption which can be obtained with the straight-line water-cooled engine is appreciably greater than is possible with the rotating air-cooled type as designed to-day.

Somewhere between the two contrasted types of engines as regards the problems of wind resistance and overall length is what is styled the V type of motor, wherein weight per horse-power is lighter than in the straight-line engine, owing, of course, to the proportionately much greater crankshaft size in relation to the number of cylinders employed. But if we consider the case of the air-cooled V-type engine, under the score of weight per horse-power, of course, it has to yield place to the rotating radial type.

Unusual Types.

Yet another type which I have produced and standardised during the past year with highly satisfactory results is a development of the V form of engine in which more than two rows of cylinders are placed on a common crankcase. The particular engine had in mind employs three rows, each of six cylinders, on

a common crankcase, each crankpin being connected to three pistons by articulated rods. In this 18-cylinder unit the centre lines of the cylinder make, in relation each to the other, an angle of 40 degrees. This allows of a very good firing diagram. This type of engine is one that is considered very promising for units of very large power. As regards weight per horse-power, it has advantages over both the V and the straight-line types of engines.

A further development of this design, in which the rows of cylinders are increased, brings us to the consideration of the fixed radial engine, which, in my mind, is one that has been sadly neglected. I feel that we shall hear a deal more about it in the near future. Several forms of these engines have been designed and made, but it may be said, in broad terms, that the success of them does not yet appear to be as great as we should be led to anticipate from consideration of the possibilities of this particular form of design.

The question of head resistance might be raised in regard to this engine, in that, when many cylinders are used, the diameter of the projected area of the power plant is increased.

Suitability of Fuselage Section.

In the case of most single-engined aeroplanes or seaplanes a fuselage of circular cross-section is admirable. It can be made large enough to accommodate the fixed radial type engine without increasing unduly the head resistance of the machine.

This is not so, however, in the case of multi-engined aircraft, in which the power plant units are placed away from the body of the machine. In these cases increase of head resistance above the minimum necessary for each power unit is the greatest disadvantage; therefore its avoidance is of vital importance. Hence for multi-cylinder aircraft the straight-line type of engine is the more suitable, particularly as the power per unit at present demanded by the builders of these machines is well within the compass of types that have been produced on the principle wherein the cylinders employed are set in a single row vertically on the crankcase.

In the circumstances in which we meet in mid-campaign it is not possible to discuss definitely the size of engine which is most likely to be adopted as standard in the near future.

Influence of Service Experience in Modifying Design.

A particular effect of the war on the evolution of aviation is the rapidity of the advance which has been and which continues to be made in the design and production both of aircraft and of engines for them. Compared with the average of enterprise in normal times, the amount of experiment that has been carried out in these directions during the last year or two is amazing, and the practical results obtained are correspondingly important. In the Sunbeam factory experimental work is held to be of vital importance, in that the discovery of anything that gives advantage over any feature of previous practice is essential for the improvement of the product standardised. Doubtless this accounts for the rapidity with which changes are made in details of design, also for the fact that the whole question of design is vastly more in a state of flux than the lay mind imagines.

Further, the experience gained by our aviators since the beginning of the war, together with the demand for the engineer to meet their ever-growing needs, have called for continuous evolution in the design of aircraft, all of which has inspired corresponding enterprise in regard to engine construction and production.

The Problem of Weight.

With regard to the question of weight, the purpose for which the particular aircraft is required is of prime importance. Obviously, in the case of the engine in a machine designed for short flights only, the consumption of fuel and of

lubricant is of less importance than the weight of the engine itself, whereas in the case of the heavier sorts of aircraft with which flights of long duration are obtained and for which great power per engine is needed, the consumption assumes much more importance than the actual weight of the engine. In these latter cases efficiency as regards the weight of the power unit has to be arrived at by taking the weight of the engine complete with the amount of fuel and oil that would be consumed in the course of a flight of, say, five or six hours' duration. Thus for short flights the rotary type of engine generally and the air-cooled varieties are apt to show up to advantage, though in them consumption may be comparatively high, because this is offset by the relative lightness of their starting weight.

From several papers that have been read recently with reference to aircraft engines it is evident that, speaking broadly, as regards weight per horse-power, progress in the design of the ordinary water-cooled type is very marked. To my knowledge, in the brief period of two years there has been obtained with this type a reduction in weight from 4.3lbs. per horse-power to 2.6lbs. per horse-power.

It follows that in designing aircraft engines a variety of points have to be considered with extreme care concerning which the builder of an engine for ordinary car service is not forced to take much trouble. This difference is rendered necessary, firstly, by reason of the amount of material employed, and secondly, on account of the comparatively light weight of the aircraft engine complete.

Valve Design: The Intermediate Stage.

The design of the engine head, the cylinders, the valves, and the valve gear is one of the cardinal features of successful aircraft engine production. Car engine design allows of the employment of the L-shaped head, or, in some cases, even of the T-shaped type, though the latter is not used to any great extent to-day for automobile vehicle practice. Undoubtedly the L-shaped head has given excellent results in aircraft engine practice in the past, but I prefer to consider that such examples really represent an intermediate stage in evolution and that they stand rather for modified or adapted car engine design than for aircraft engine design proper. In point of fact, high efficiency is got with this type of head only by the use of a special design of valve cap that makes provision for the maximum surface of the engine head being served with water by the cooling system. In other words, you employ a form of duplicated valve cap, the removal of the upper and outer member of which reveals a space for water to circulate beneath it when the engine is working. At the bottom of that water is the valve cap proper. Further, to get the best results, it is needful to machine as much of the surface of this transition type of engine as possible.

The Number of Valves per Cylinder.

For standard car work one exhaust and one inlet valve per cylinder have sufficed for general practice to date, whereas present-day demands on aircraft engine designers are so great that any attempt to attain the requisite degree of efficiency by further exploiting such a scheme of design would lead inevitably to failure. The necessity for running aircraft engines for long spells at either the maximum or a very high output without impairing the efficiency of the machine by distortion or pitting of the valves, which assuredly would occur with the ordinary design employed for car service, and so forth, has compelled the devotion of much thought and a wealth of experiment to the probelm, as a result of which it appears to be accepted as established that the multiple valve system is a necessity.

In this connection, to achieve maximum output, I favour two exhaust valves per cylinder and two inlets.

The horse-power obtained by engines designed on this principle and now

standardised would appear to justify this conclusion. For example, a six-cylinder engine embodying this feature of valve design and having a cylinder bore of 8½ mm. with a piston travel of 156 mm., develops 153 horse-power at 3,000 crankshaft revolutions a minute. That is equivalent to a mean effective pressure of 134lbs. per square inch. At 2,000 crankshaft revolutions a minute it represents a duty of 21 horse-power per litre capacity.

Among the advantages of the four valves per cylinder scheme are that a good shape of engine head is obtainable with it as well as the best sparking plug position, because that is vertically in the centre of the head. The inclination of the valves necessary for putting them into place allows of ample water-jacket space being provided round each valve and at the base of the sparking plug.

Three Practicable, More than Four Undesirable.

Judged by achievement to date, any other combination of valves per cylinder will not give quite the same degree of efficiency. Take such variants as three valves per cylinder on the principle of two exhaust and one inlet valve. While giving perhaps a better-shaped, because circular, head that can be machined practically all over, this combination practically precludes any other sparking plug position than in the side of the barrel. For obvious reasons, such an arrangement has many disadvantages. Nevertheless, the three valves per cylinder scheme gives quite notably good results when exploited in certain ways and for certain special purposes. This may be judged from the fact that the diameter of the inlet valve can be made large enough to-day to give a very high horse-power per litre capacity without involving serious trouble, such as would arise from heat effects, and so forth.

The use of more than four valves per cylinder is undesirable. It seems hardly possible to place them efficiently, leaving an even jacket all round each valve, without the employment of very complicated gear. We have an example of this in the Maybach engine, which has three exhaust and two inlet valves per cylinder. In this scheme little water space is provided between the valve seats, while the sparking plug is, besides, set horizontally in the side of the cylinder barrel.

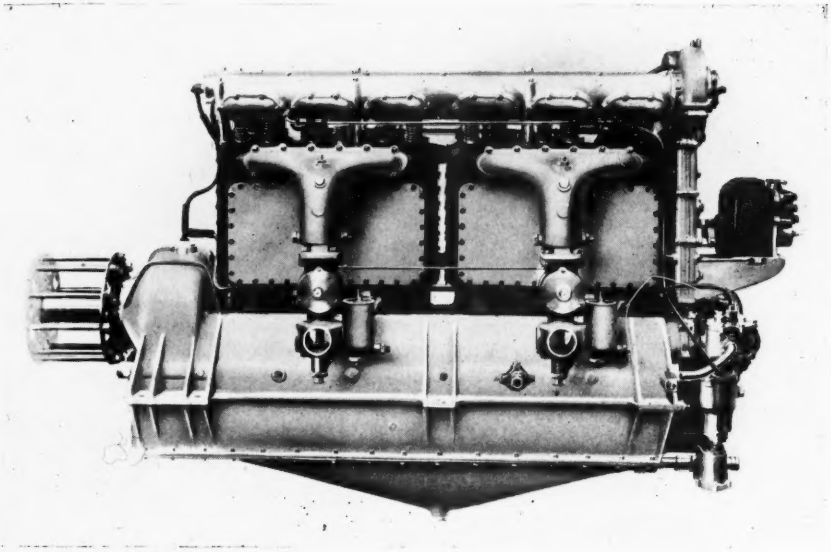
Undoubtedly in any type of engine it is a gain when the surface of cylinder head can be machined. This is not possible, however, with the four valves per cylinder design. Hence in that case a compromise is achieved by making the surface as small as possible and by finishing it as much as possible by hand with files, scrapers, and so on.

The Matter of Material.

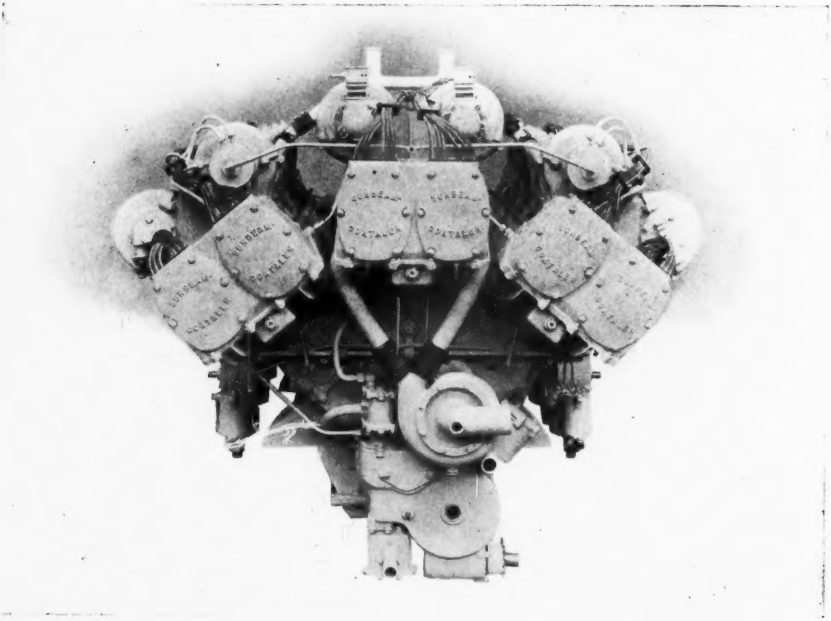
In regard to the material of which the cylinders are fashioned, a tribute is due to the metallurgists and manufacturers on account of the great improvement in the cast iron available for cylinders of recent years, the very high tensile and good ductility qualities of it being particularly notable.

Of course, the advent of aluminium alloy as a material for cylinder castings marks an important stage in the advance of aircraft engine design and production, the rate of progress obtainable by the designer being determined primarily and always by the materials that are available to him. A series of experiments carried out by me over a period of more than twelve months has proved the superiority of aluminium alloy for this work. It has established the fact that it can give completely satisfactory results when the precisely right alloy is handled with knowledge and is employed on tested designs. Thus a 12-cylinder aircraft engine of 92 mm. bore and 135 mm. piston travel has run at full power for 100 hours without an involuntary stop or untoward incident, otherwise with complete satisfaction.

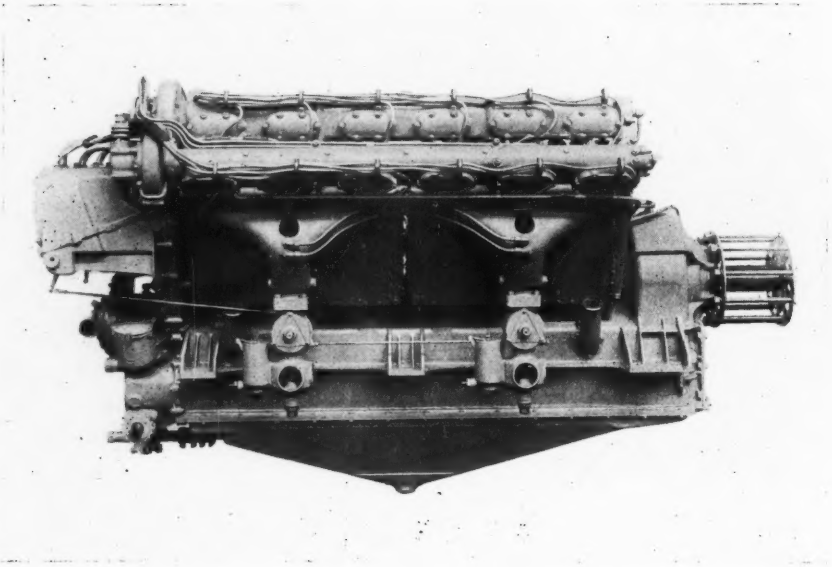
With that sort of confidence which is founded on reason, as the result of cumulative experience on the one hand and further research on the other, particularly with a view to rendering these special alloys capable of being cast



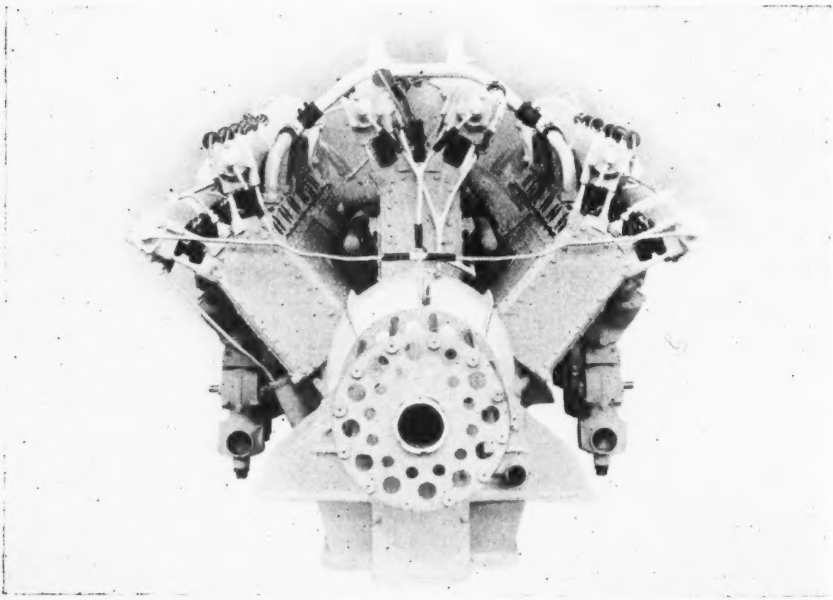
SIDE VIEW OF 160 H.P. SUNBEAM-COATALEN 6-CYLINDER AIRCRAFT ENGINE.



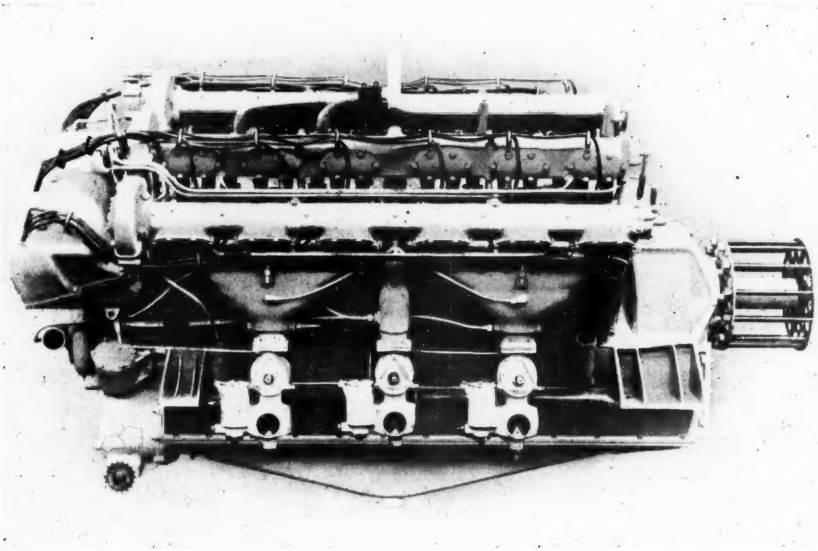
MAGNETO END VIEW OF 475 H.P. SUNBEAM-COATALEN 18-CYLINDER AIRCRAFT ENGINE.



SIDE VIEW OF 350 H.P. SUNBEAM-COATALEN 12-CYLINDER AIRCRAFT ENGINE.



PROPELLER SHAFT END VIEW OF 475 H.P. SUNBEAM-COATALEN 18-CYLINDER ENGINE.



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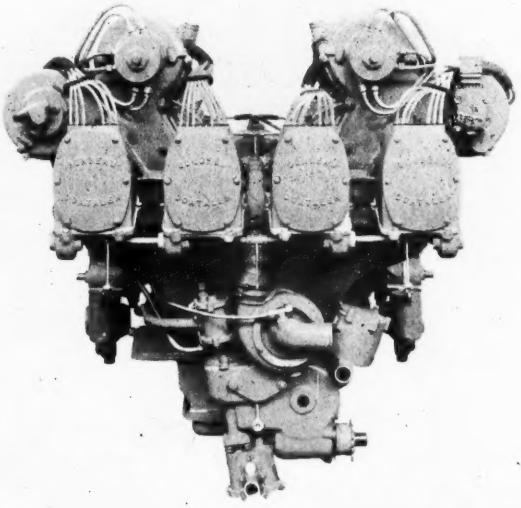

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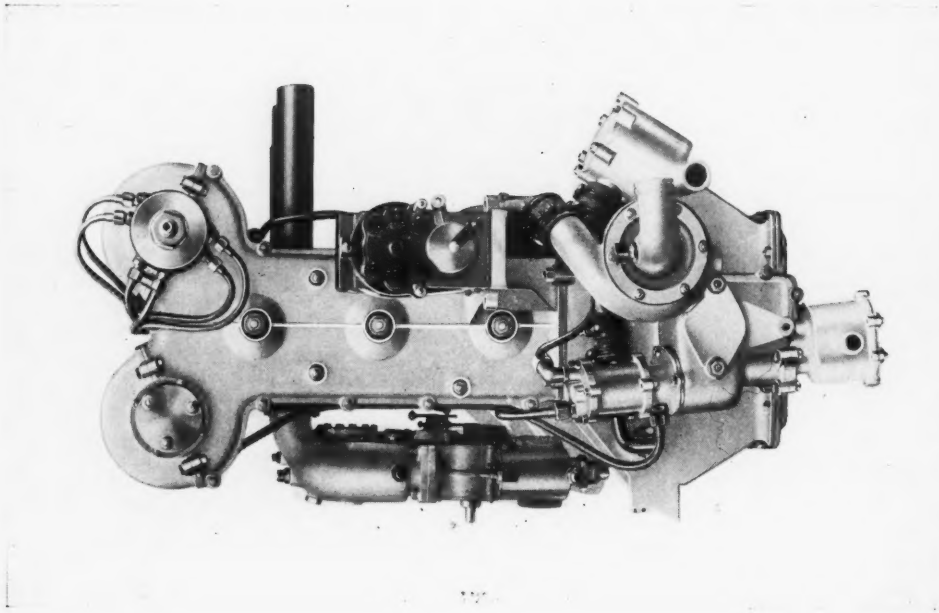
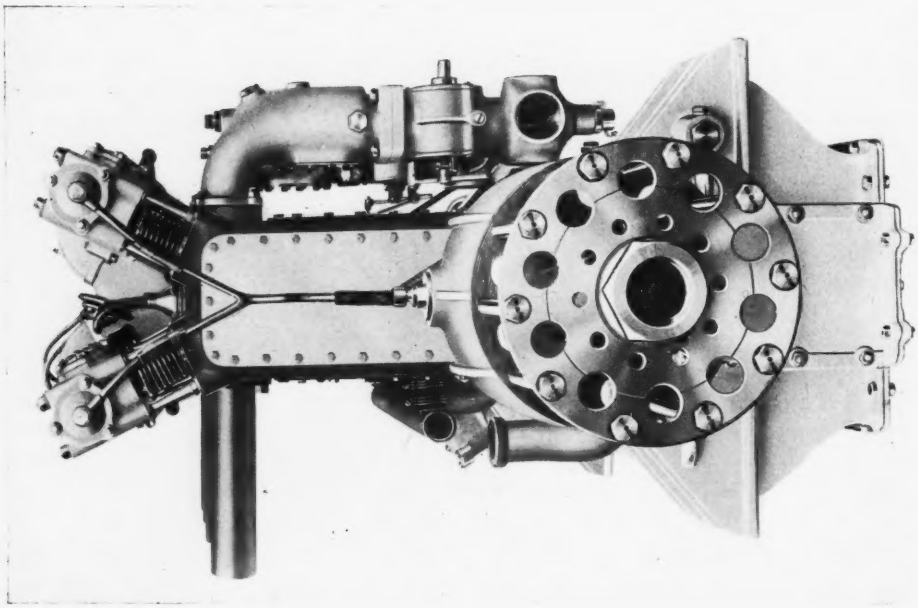


MAGNETO END VIEW OF 160 H.P. SUNBEAM-COATALEN AIRCRAFT ENGINE



PROPELLER SHAFT END VIEW OF 160 H.P. SUNBEAM-COATALEN 6-CYLINDER AIRCRAFT ENGINE

PROPELLER SHAFT END VIEW OF 160 H.P. SUNBEAM-COATALEN 6-CYLINDER AIRCRAFT ENGINE

MAGNETO END VIEW OF 160 H.P. SUNBEAM-COATALEN 6-CYLINDER AIRCRAFT ENGINE

easily in the factory, we may look for both speedy and consistent improvement in those varieties that will be available for this highly specialised work. Though we are merely on the threshold of realising the possibilities of aluminium alloys for cylinder castings, it cannot be doubted that within a brief period they will be recognised as the standard materials for this work, cast iron being discarded in favour of them.

Progress in Castings and Alloys.

The importance of the matter is realised by the aluminium founders of the country. It is encouraging to note the relatively great amount of enterprise being displayed by them in this connection. At the present time the more important aspects of the case embrace the effect of heat on the strength of the given alloy as well as the coefficient of expansion of it. In the manufacture of aircraft engines more and more use is being made of aluminium alloys of varied analyses, each to suit some particular condition of work. With a continuance of advance in knowledge of heat treatment, and so forth, there is no gainsaying that this material will be employed for aircraft engine construction to a greater and yet more great extent.

Aluminium Alloy Pistons.

Under this head it falls to be observed that for about two years I have standardised aluminium alloy pistons with excellent results. Even when they have been made with a green sand core no real trouble can be said to have been experienced with them. A point to note, however, is that the greater clearance needed when the aluminium alloy piston is cold represents a disadvantage in comparison with cast-iron pistons, which has not been overcome yet.

Special Alloys in Place of Gunmetal for Oil Pump.

Another special alloy is being used in place of gunmetal for the construction of the oil pump employed for the forced feed lubricating system. These details are of the gear-wheel type. As regards both strength and bearing qualities the alloy employed has proved to the full as satisfactory as the gunmetal used formerly.

Problems Presented by New Materials.

Before quitting the subject of materials in general, at this juncture it will be convenient to pass the general situation in rapid review. Manufacturers have been called on to make immense efforts in the matter of supplying a wide variety of materials for multi-cylinder aircraft engines. They have met and mastered right ably the usual sequence of difficulties that materialise whenever man attempts to break fresh ground. In particular, the high-tensile steel stamping now being supplied, for instance, for a six-throw crankshaft for a 500 horse-power aircraft engine is a splendid example of the steelmakers' craft to-day. There would have been no call for it had not the advent of war made it necessary on a sudden for us to standardise in these islands aircraft engines of high output. Of course, the necessity for using unprecedentedly high-class materials for these constructions has presented alike steel makers and alloy producers, as well as the engine makers' machine shops, with a series of fresh problems which have had to be overcome detail by detail before it has been possible to obtain that degree of success which is necessary ere any given product can be regarded as a practical proposition.

Briefly, there has had to be an all-round improvement in method. More scientific control has had to be exercised and procedure elaborated. Obviously, there has been a call for devoting the greatest attention to detail, since it will not suffice merely to employ more expensive workmanship and higher grade materials. In regard both to design and to procedure, each part must be accorded, besides,

greater attention to detail than any that has sufficed for car engine design and production to date. In what direction this extra study is needed is discovered, of course, by the laborious process of experiment. After that knowledge has been attained, when it comes to standardising practice in the shops, very special attention has to be given to avoid points likely to start fatigue flaws. At this stage these are a prolific source of trouble in the production of aircraft engines. Each man has to be trained to give the correct proportion of attention to the various details of his job; therefore a more highly-skilled class of labour is needed. Your individual worker must know precisely what function his particular task and the part which he is engaged in fashioning plays in the scheme of the complete and, necessarily, at present somewhat complicated aircraft engine. Though only a matter of lightening, such details as boring parts, with which the car engine builder is not concerned, have to receive more than ordinary intelligent and conscientious attention during manufacture.

Connecting Rod Forms.

Of course, both the radial and the V-type aircraft engine have introduced problems of design, the solution of which does not appear to be unanimous yet. In these varieties of engine design you have more than one piston attached to each crankpin. The diversity of opinion concerns the means by which this is done. Naturally the design of eight and of 12-cylinder standard engines for car service, such as our American friends are producing in large quantities, does not call for the same amount of care in this regard that is essential in the case of the more heavily loaded aircraft engine. The original form was that in which the end of the subsidiary connecting rod is mounted on the outside of the main connecting rod. This, however, makes relatively a very heavy big end not suitable for engines from which is demanded the high duty necessary for aircraft service, as the wear of the big end is controlled by its weight and oil pressure.

A lighter construction is achieved by the link-rod method, wherein the subsidiary member is attached to the main one by a pin placed as near the centre of the crankpin as possible. This arrangement has the additional advantage of rendering it considerably simpler to take up any wear in the big end bearings. For obvious reasons, in the former type that operation is not a very easy one. The latter type, however, introduces a further problem. Owing to the centres of the bottom pins of the subsidiary rods not being coincident with the centres of the main pins, the movement of them is along an elliptical path in contradistinction to that of the crankpin, which, of course, is circular. The ellipse has the effect of altering the stroke of the piston attached to the subsidiary rod. The extent of the variation depends on the angular position of this pin with the centre line of the main connecting rod, the distance from the main pin to the rod pin remaining constant.

The Lesser of Two Evils.

It will be readily appreciated that the length of the piston travel can be corrected by tilting the axis of the ellipse in such a manner as to give the precise stroke desired. Owing to its change of velocity, also to its greater stress on the main rod due to the reversal of that rod during the firing stroke, this would have the further effect of altering the acceleration of the piston attached to the link rod. In going into this question the author came to the conclusion that the variation of the stroke was the lesser of two evils. The design of engine corrected for stroke, showing the variation in piston velocity, is illustrated clearly in the accompanying diagrams.

The design adopted in certain radial engines, in which gear-wheels are used to maintain the correct position of the link-rod pin, is a very clever method of overcoming these difficulties. Nevertheless, it makes for relatively heavy construction and tends to provide a further cause of mechanical failure.

In the case of the three-row 18-cylinder 500 horse-power Sunbeam-Coatalen aircraft engine, the design of connecting rod arrangement in regard to the link pin details is such that the centre row of pistons that are attached to the main rod of each series have a travel of 160 mm., while each of the pistons of the two side rows of cylinders has a stroke of 168 mm.

As it is essential to employ as light a connecting rod arrangement as possible, the question of loads on the bearings becomes of great importance.

Despite the fact that one engine of great power has been designed with balance weights and has certainly given satisfactory results as standardised, and that, of course, these can be used in car practice, nevertheless such accessories are practically precluded in the case of the latter-day aircraft engine on consideration of the weight per horse-power of the given engine.

Lubrication.

As has been indicated, lubrication is prominent among the matters that illustrate the difference between car and aircraft engine design and practice. While everything possible is done to reduce the bearing pressure in the aircraft engine, nevertheless in practice it is found that the best design is one in which less bearing surface is allowed than obtains in the case of the car engine. This is rendered practicable in the aircraft engine by giving more careful consideration to the problem of lubrication. Thus a pressure in the oil circuit up to 100lbs. to the square inch is being used. This ensures an excellent condition of bearings, while the design of the oilways in the bearings themselves is such as to prevent excess of lubricant getting to the pistons.

Because an aircraft engine has to carry its supplies of lubricant aboard the machine, obviously it is only less desirable to ship an excessive amount of oil for any given flight than it is to carry too little. Moreover, the fact that the engine bearing pressure is much higher than in motor-car practice, and as much more heat is generated and has to be absorbed by the film of oil on which the lubricated surfaces are floated, renders it impossible to carry the oil in the base-chamber, as in ordinary motor-car engine practice. In aircraft engine design you aim at achieving the maximum economy of lubricant consumption in combination with maintaining effective pressure in the system by preventing the oil becoming too hot and thereby losing its viscosity. Therefore you design an aircraft engine with what is styled a dry base-chamber, placing the oil supply in a tank set somewhere else in the machine, probably in the slipstream of the propeller, so that it may be exposed to the maximum draught and its contents cooled as rapidly as possible, to be served again to the engine by the intake pump in connection with the pressure system. The relief valve returns all excess oil to the tank and the expelling pump at the bottom of the base-chamber withdraws all the lubricant that, having escaped through the connecting rods and served the details that are oiled by the splash system, returns as filtered residue to the tank, where it is cooled as quickly as possible, and therefore made ready to be passed into the engine system again. Experiment has proved that for a long run under full load it is necessary to force the oil through some sort of cooler placed in the air-stream in a similar way to the radiator. Likewise the design of base-chamber must be such as to act as a cooler of sorts for the oil passing down from the crankshaft and other details to the sump. Of course, the high pressure to which reference has been made is needed in connection with the main bearings, the crankshaft, and the connecting rods only. In this regard a great advance has been made recently by providing a supplementary pump of small capacity which serves to lubricate the minor parts of the engine that do not need high pressure. This ensures that these minor parts get each a proper proportion of lubricant without supplying it at the possible expense of the main bearings and without receiving an excess supply which might make a dirty engine.

Points about Cooling.

From lubrication we pass to its obvious corollary, cooling. Considerable divergence of opinion still obtains among engine builders and radiator makers concerning water-cooling. The author is of opinion that, because increased flow of water would allow of the employment of a smaller radiator surface, that line of development is likely to be considered in the near future.

Another important matter concerns the rendering adjustable of the cooling capacity, or the surface, to suit variations of climatic conditions and of altitude. Assuredly, this is highly desirable. The conditions under which aircraft are being used to-day renders such a development well-nigh imperative. The use of radiators of relatively less size, such as is to be expected from speeding up the flow of water, should afford a notable advantage in this particular connection.

It is to note that air-cooling is coming into favour increasingly. The introduction of aluminium alloy in the manufacture of the cylinders has exercised a marked effect in regard to this tendency. It would appear that to date relatively very little has been done with multiple valves as applied to air-cooled engines. Yet in the matter of the advantages of employing multiple valves to the number of four per cylinder it would seem that the gains of this system as applied to water-cooled engines should obtain equally in the case of the air-cooled varieties. Further, the small valve is likely to give more satisfaction in air-cooled than in water-cooled engines by reason of the time factor in the conductivity of the heat from the individual valve to the adjacent parts of the cylinder. For these reasons, among others, in the near future air-cooled engines of larger power may be expected to materialise. Certainly this type is very promising.

Carburation at the Cross Roads.

As applied to aircraft engines, the proposition of carburation stands to-day somewhat on a basis of compromise. For this reason experiments now being carried out are directed towards obtaining more efficient and economical carburation. The tests made on Sunbeam-Coatalen aircraft engines at the manufacturers' works at Wolverhampton have shown a petrol consumption of .52 pints per horse-power per hour, coupled with an oil consumption of .022 pints per horse-power per hour. It will be readily agreed that this stands for a distinct advance on consumption by engines using ordinary type carburettors so recently as at the beginning of the war.

Nevertheless, there is room for a deal of improvement yet. Perhaps this is not so much from the maker's point of view as from the pilot's. One refers more particularly to the supply of petrol to the carburettor itself, for instance, when the aircraft is rising rapidly or stalling. In the case of military aircraft, particularly, there arises the problem of carburation while the attitude of the engine is being varied in all manner of directions owing to the manœuvring necessary while fighting an enemy.

Anticipated Great Alterations in Design.

When this war began there was relatively little fighting in the air, and the average flying was done at anything from 4,000 to 6,000ft. To-day our airmen rarely go over the lines at less than 16,000ft., and fighting has taken place certainly at altitudes of 21,000 and 22,000ft. Accordingly it will be realised that at the outset of the campaign the problem of altitude was not thrust to such an extent on the attention of the designer and the manufacturer because such modest heights were deemed sufficient for aerial reconnaissance and other work, whereas in the interval it has become imperative to navigate the air at such vastly increased heights that the difference in atmospheric pressure can be ignored no longer for the sufficient reason that the altitudes in question could neither be attained nor maintained if the problems presented had not been solved, at least in part, already. They concern both carburation and engine compression, as

well as the matter of cooling. As to carburation, the influence of altitude is quite the most important matter now engaging the attention of designers.

Forced Induction and Increased Compression.

In dealing with the situation the correct line of attack would seem to lie in the direction of forced induction, though under present conditions perhaps it were unwise to state the manner in which experiments are being carried out towards this end. Suffice it that the problem is abundantly obvious and that great alterations in design are to be expected. Doubtless they will mark yet another point of very great divergence of design from that employed for engines for car service. It is beyond dispute that in the near future the two types must become more and more pronouncedly contrasted.

The alleged method of increased compression exploited for Zeppelin service as a means of tackling this problem is peculiarly suitable for airships owing to it not being necessary for those craft to climb by mere engine power, also to the fact that a major part of their work is done at high altitude. It is possible that the engines in which small change in atmospheric pressure is allowed for are used entirely for work at high altitudes, and have a much higher compression than those which are employed for manœuvring Zeppelins near the ground.

Of course this question of compression is interconnected with the problem of carburation; hence we must also regard altitude as a governing factor in the design of the latter-day aircraft engine. In the last few years considerable advance has been made in the degree of compression standardised successfully. Thus engines with a compression ratio as high as 6 to 1 are running satisfactorily at sea-level to-day. That, however, has been rendered possible only by evolving such a combination of features as valves of suitable design, diameters, and openings, and by going very scientifically into the matter of cylinder head design.

Three Years' Progress at a Glance.

Assuredly it is interesting to compare present-day achievement with results of, say, three years ago, as instance those obtained at the Naval and Military Aeroplane Engine Competition in 1914. As a result, it will be found that then the use of aluminium was practically confined to the crank case only. Its application to the construction of pistons and other small parts of the aircraft engine was not known. Further, we find that the maximum mean effective pressure was approximately 106.5lbs. per square inch, and that the average fuel consumption was .6 pints per horse-power per hour. The weight of the engine with fuel for a run of six hours' duration varied from 9.55 in the case of the rotary air-cooled variety to 11.27 in that of the vertical water-cooled type.

By contrast, to-day the mean effective pressure standardised has been increased to 135lbs. per square inch, measured from the brake horse-power and, in some cases, actually through the reduction gear. At the same time fuel consumption has been reduced to .52 pints per horse-power per hour, while the weight of the V-type water-cooled engine has been brought down to 6lbs. per horse-power per hour with fuel and oil for a six hours' effort, all of which I hold to represent a notable rate of progress achieved in the brief period of less than three years.

As regards the heart of the aeroplane, its power plant, there is no sound reason for adhering to the general attitude of the British public, which is to decry home achievement, praise all foreign endeavour and, notably, to set German effort on a pedestal as something unapproachable. How erroneous is that idea when applied to the aircraft engine proposition you may gather from a single instance. Take the latest 6-cylinder water-cooled German Mercedes aircraft engine of the four valves per cylinder type. Without radiator and water that

weighs 3½lbs. per horse-power. Now in this country there are designed and produced water-cooled engines which, without water and radiator, weigh 2.6lbs. per horse-power, nearly one pound per horse-power lighter than the best known German performance.

Installation of Aircraft Engines.

I feel that the present occasion is opportune for touching briefly on the installation of engines in aircraft. As you will be aware, a very high percentage of engine trouble on active service is due to failure of some detail or other in connection with the installation. Therefore, it cannot be too widely known that all petrol piping should be insulated as absolutely as possible from the effects of vibration. The first rule should be to support all such piping throughout its length. Take, for example, the pipes as they pass along the fuselage details between the engine and the tank. It should be impossible to shake these by the hand at any point. If, on going over them, you can make a slight movement, there you should provide additional support. Again, wherever there is a change of unit as, for example, where the pipe is taken from the fuselage to the engine bearer, there that pipe should be cut. Further, you should always provide steel liners for the nipple. These liners should be proportionately very long so that the Durit will be held on the nipple itself. Reliability in service depends enormously on the giving of due attention to little details of this character, which are so obvious that it seems almost superfluous to mention the matter. Indeed, it would be were it not for the fact that these first principles of aircraft engine installation, if I might so call them, are neither generally understood nor practised yet.

Method of Rating.

From the inception of the movement several methods have been proposed for rating petrol engines. At the stage at which we have now arrived in constructing power plant for aircraft service some figure seems to be needed which will give a notion of the efficiency, or horse-power output, of an engine in relation to its size. To-day the mean pressure is used often for this purpose; but in my view that is neither convenient nor can it be arrived at easily.

For this reason I wish to propose that the horse-power per unit capacity obtained from any given engine be taken as the standard for preparing the different "duties" of engines. Of course, the figure obtained is proportionate to the mean effective pressure, but doubtless it will be agreed that it is more convenient.

The capacity taken would be the capacity per cylinder multiplied by the number of cylinders and by the number of complete cycles per minute, but to serve the aim in view the horse-power per litre engine capacity per 1,000 cycles, otherwise per 2,000 crankshaft revolutions a minute, is proposed.

In his Presidential Address to the members of the Institution of Automobile Engineers in October, 1916, Mr. Legros gave some very interesting figures comparing petrol engines by means of the equivalent radius or torque of the crankshaft divided by the weight of the given engine. In connection with the aircraft engines cited, I have reduced these figures, and find the results vary in the range of 18 to 21 horse-power per litre capacity at 2,000 crankshaft revolutions a minute. I deem the latter figure the maximum likely to be obtained at sea-level with water-cooling by the present design of engine without the use of supercharging, measuring the duty at the propeller.

EIGHTH MEETING, 52nd SESSION.

A general meeting of the Society was held on Wednesday, the 18th of April, 1917, at 8.0 p.m., in the Theatre of the Royal Society of Arts, Adelphi, London, W.C. There was a large attendance.

The chair was taken by Sir CHARLES PARSONS, Bart., K.C.B., D.Sc., F.R.S., who briefly introduced the lecturer.

Dr. W. H. HATFIELD read a paper on "Steels Used in Aero Work."

A discussion followed.

Professor CARPENTER, M.A., Ph.D., in opening the discussion, expressed his great appreciation of the valuable lecture delivered by Dr. Hatfield. He was particularly pleased to note the importance which had been given to the iron-carbide diagram as the basis upon which steels and their treatment should be studied.

The special opportunity which the smallness of aero parts offered for careful and distinctive heat-treatment was also a matter which he (Professor Carpenter) thought merited the attention of the industry, and he endorsed strongly the lecturer's views with regard to the importance of the heat-treatment, hardening and tempering operations receiving the attention of men of the highest skill.

He was inclined to think that the lecturer had rather given the impression that the elastic limit determination was more troublesome than was really the case. Owing to the importance of the truly elastic range, he considered that determinations should be made with much greater frequency than hitherto, and he thought that the information so obtained would be of primary importance to the engineer.

The lecturer had pointed out the variety of materials sometimes used for one particular purpose, and he thought he could select no better instance than pistons. The Professor here discussed in some detail the apparent anomaly existing in the use of such dissimilar materials as had been described in the lecture.

He found that the 25 per cent. nickel steel was discussed in the paper, and he quite agreed with the lecturer's remarks that such material behaved somewhat inconsistently and that this particular steel needed thoroughly investigating. The iron-nickel series was a very interesting one, and if the diagram were studied it would be seen that as regards non-magnetic properties only a limited range of composition was permissible. Again, with regard to resistance to corrosion, further research work would probably result in a more complete understanding of the material.

Mr. FOWLER (Royal Aircraft Factory) raised the question with regard to the impact test. He stated that although when he first became connected with aeronautical work he had some doubt upon this, yet a closer connection with these tests had convinced him that it was of the greatest importance, and his experience pointed to the connection which exists between low impact results and failures of cranks.

He called attention also not only to the light specific gravity, but also to high conductivity of carefully-selected light alloys for certain engine work.

He would be glad if Dr. Hatfield would state what was the effect of cold work on the properties of stainless steel and whether any value would be derived from this class of material for rafwires.

Colonel BAGNALL-WILDE :

Factors of Strength.

I quite agree that the actual values of the factor, call it what you will, from the design point of view should be placed on a more definite basis; unfortunately, however, modern conditions of warfare render it almost impossible to do so. The best that can be done is being done.

I am in disagreement with many designers who base their factor of safety on the maximum breaking stress, I think the yield point alone should be considered. The relation between maximum and yield is well known with high alloy steels and is well determined in specifications.

Crank Cases.

I do not see how it is in any way possible to arrive at Dr. Hatfield's conclusion, *i.e.*, that crank cases and parts should be rigidly fixed; he certainly does qualify it in that he only proposes to retain such rigidity as will prevent undue bending in the crankshafts. It must be borne in mind, however, that an aero engine, as at present designed, cannot in any way be regarded as a rigid structure.

I quite agree with Dr. Hatfield that H section connecting rod is preferable to the hollow tubular design, though the latter possesses possible advantages with regard to ease of manufacture.

Pistons.

As regards pistons, I think it is pretty well established that aluminium alloy gives the best results. In the early days an impression existed that aluminium was used for pistons on account of its lightness. I am personally of the opinion that the aluminium piston, even if heavier than cast iron, has so many advantages on account of its superior conductivity that its weight need not enter into consideration.

Mechanical Testing.

It is difficult to avoid the somewhat controversial question of the determination of yield and true elastic limit. I congratulate Dr. Hatfield on placing the matter so clearly before us. I do think, however, that too great stress is, in many cases, laid on the difficulty of correctly determining the yield. I quite agree that it is advisable to have the load run back about the yield point to obtain the permanent set, but in commercial testing, how often is there time to do this? I am of the opinion that the yield point can, within reasonable limits, be ascertained, except in the case of special alloy steels, so heat-treated as to give something over 70 tons ultimate stress. No inspector would reject a high tensile steel because the yield as measured by him is, say, 2 or 3 per cent. below the bottom limit of the specification. If the yield is given at, say, from 55 to 65 tons per square inch surely there is a reasonable margin for possible errors in reading the yield, especially if the inspector is allowed discretionary powers. I am further of the opinion that an incorrect interpretation of the reading shows itself when co-related with the other figures. It may look bad on paper, but to those who are accustomed to reading testing results the answer is, more often than not, obvious; if not a re-test can be made.

I quite agree that the bending test is a very valuable one, but in the majority of cases it requires considerable knowledge and many years of experience to correctly interpret the result obtained.

The impact test has been much abused. I entirely disagree with Dr. Hatfield when he states that with regard to determining brittleness the usefulness of the test remains to be demonstrated.

The Izod or Charpy tests, both of which are of similar nature, have been proved by me capable of determining brittleness, in cases where no indication of such a condition has been given by a tensile test. The brittleness, as indicated

by the Izod, has been verified by me again and again. I was formerly of the opinion that a low Izod figure on which a rejection resulted might not mean that the steel was unsound, but an unsound steel undoubtedly gave a low Izod. I now believe this latter statement only.

I agree with Dr. Hatfield that the tests are sensitive, but if he will examine specifications I think he will find that a minimum figure only is given for required Izod test results.

I agree that the results obtained very often are found to be in the neighbourhood of double the minimum figure, but I have also found that steel which gives an Izod test figure below the minimum justifies rejection for brittleness.

A typical example. Certain steel used for making nuts and bolts was found to contain sulphur and phosphorus in excess of .1; such steel invariably failed to give any higher reading than 2 to 3 ft. lbs. on the Izod, whereas a steel of exactly similar composition and with similar treatment, but with sulphur and phosphorus below .06 per cent. gave an Izod test figure of over 40 ft. lbs.

In correctly interpreting the Izod readings the examination of the fracture is certainly desirable, but provided the specification figures, as stated above, are given as a minimum, whether the results are double the minimum or are the minimum figures is immaterial from a commercial standpoint.

The disadvantages of the Izod and Charpy tests, especially the latter, lie in the amount of work required to machine the specimens. Dr. Hatfield mentions the sharpness of the "V" notch in the Izod. I proposed and have now adopted a notch with a rounded root; this practically enables greater accuracy to be obtained in machining, but in my experience this is not so essential as is generally considered.

I consider that the early troubles with the Izod were due to lack of knowledge or, I should say, experience, as to how to obtain a figure which would pass specifications.

It is only recently that I became aware of the fact that in the case of an alloy steel, quenched from 820 and tempered from 400 the Izod drops. The drop is a very interesting one; it suddenly starts just before 400 and drops some 30 or 40lbs. and suddenly rises again as the tempering temperature is increased some 25° or 50° above 400.

I think I may safely state that nobody has yet published a satisfactory explanation of this most interesting fact. We well know that the temperature concerned is intimately connected with the magnetic change, but we do not know that a steel tempered at that temperature which will give this sudden drop in impact is an unsound steel.

I think I can only say that owing to the doubt which exists I prefer a steel tempered at a temperature either above or below what might be termed the critical Izod tempering temperature.

I am sorry that Dr. Hatfield, apparently, intends to convey the impression that the Shore Scelescope can be compared to the Brinnell machine. In my experience the scelescope is comparatively useless except for one purpose, and that is the ascertaining of the surface hardness of case-hardened parts; even then I prefer to test with a file and trust to personality.

With regard to the Brinnell machine, much misconception exists as to its use. It is generally misunderstood, in that the readings are not co-related with the tensile strength of the bars as tested. The Brinnell figures only give the same indication of the tensile strength for steels off the bars of similar composition and similarly heat-treated. I use the Brinnell freely as a check test, but its use should be, and is so far as I am concerned, primarily confined to the following:—

The tensile test piece is pulled to represent, say, 25 bars. The Brinnell figure is then taken on the individual test piece. The Brinnell figures taken on the 25

bars must, in order that these bars may be passed, conform to the Brinnell figure obtained on the tensile test piece.

As regards alternating stress tests and the Stanton impact tests. The drawback to these methods lies in the time taken to carry out the test. It is essential for commercial work that the test should be such that the minimum machining is involved and the minimum time taken in carrying out the test. I am of the opinion that the results obtained with the Izod are far more reliable and, when taken in conjunction with the appearance of the fracture, far more valuable than any of the alternating stress tests. Though I am of the opinion that the latter may possibly be desirable, at the present time insufficient data exists to enable a correct interpretation to be made, in other words they are of scientific value but are not yet commercial.

In general, as regards all testing, it must be borne in mind that testing is not carried out commercially from a research point of view and testing must be purely commercial. A man wants to buy steel and he wants to know that the steel maker supplies him with something reasonably in the neighbourhood of what he requires.

I think that, at the present time, the buyer may safely rely on tests which give Ultimate Stress, Yield, Elongation and Reduction of Area, an impact figure and in some cases an analysis within fairly wide limits.

I am interested in the list of steels Dr. Hatfield puts forward as suitable for aero construction. I have recently expressed the opinion that five steels will practically fulfil the majority of requirements. I am afraid this statement has been rather misunderstood, because I have been imperfectly quoted. What I mean is that five steels will meet some 75 per cent. to 80 per cent. of the tonnage required. Dr. Hatfield therefore proposes only three steels to meet my five, *i.e.*, 1, 2 and 3. The air-hardening steel is somewhat under a cloud under present conditions and it need only be used, in my opinion, in exceptional cases. His steels 5, 6, 7 and 8 are, of course, required for certain purposes, but only in small quantities.

The steels I suggest are:—

1. .15 Carbon (Case Hardening).
2. 3 per cent. Nickel (Case Hardening).
3. .4 Carbon (Steel for Forging).
4. 3 per cent. Straight Nickel.
5. 3 per cent. Nickel Chromium.

As regards carbon steel, it might be assumed from Dr. Hatfield that the acid Bessemer process was the most important for the production of carbon steel for aero construction. I am afraid I cannot altogether agree; although Bessemer is most excellent for a variety of purposes I can hardly agree that even the acid Bessemer process is the most suitable for the production of aero steel.

As regards air hardening steels—I agree that they are extremely important and that *when* they are more completely understood they may be of more use than at present, but I do consider that owing to their not being understood, with which statement I think Dr. Hatfield will agree with me, it is not advisable to press their use at the present time. They are certainly difficult to work.

I quite agree with Dr. Hatfield as regards sheet steel, in that there is far too great a tendency to overdo cold work. This is a matter to which designers should give great attention and they should draw up specifications including clauses whereby all such parts are heat-treated after either bending or welding.

Steel Tube.

Dr. Hatfield has touched on many subjects and I much regret he has not sufficient space to give us more details of that most important industry, *i.e.*, the production of tubing. I suggest that the matter could well form the subject of a separate paper.

Cast Iron.

I have experienced very grave difficulties in obtaining castings uniform in composition. As I have stated before, pig iron before the war was ordered as a proprietary brand and was then fairly uniform in composition; this is not so now. From analyses recently made it is clear that it is very difficult indeed to get castings other than extremely high in phosphorus. The founders did not welcome the idea of using hematite pig to assist in reducing phosphorus content.

I well realise it is difficult for the average founder to analyse the consignments of pig which he receives, but I would impress on all concerned the necessity for so doing and have expressed my willingness for the analyses to be made by my staff. It is only fair to say that recently considerable improvement has been made.

Stamping.

I quite agree with Dr. Hatfield that the average designer either has ignored the stamper, or, judging by the drawings he produces, possesses no knowledge whatever of this most important and valuable branch of the industry. I have elsewhere expressed myself at length on this matter.

Machining Properties.

Again I agree with Dr. Hatfield as to the small amount of knowledge apparently possessed by manufacturers as to the machining qualities of steel of varying compositions. Firms who turn out nuts and bolts and automatics appear to know very well that mild steel with sulphur and phosphorus in excess of .1 per cent. and even with .15 per cent. saves them money—cuts like cheese and makes full threads. The result is certainly not fit for use in aeroplane production; it is brittle and in fact the heads of the bolts made of such material fall off when tapped with a light hammer.

Heat Treatment.

Works managers must decide before they start work as to whether they require steel in the normalised conditions for rough machining, then to heat treat it and finish by final grinding, or whether they are prepared to face machining the material in its heat treated condition. The whole question is primarily one of time. In very few cases are steels required for aero production in such a condition that they cannot be machined, it takes longer and is more expensive in tools to deal with steel in the heat treated condition; on the other hand, rapidity of machining is obtained when the steel is in the normalised condition, but time is lost in subsequently heat treating and, what is more serious, unless this process is carefully carried out, warping and skellering may render the part too much out of truth for final grinding.

I quite agree with Dr. Hatfield as regards the necessity for classification. Much work in this connection has been carried out by the Engineering Standards Committee; the detail investigation being conducted by the Joint Steel Research Committees under the auspices of the Institute of Automobile Engineers.

Mr. FOWLER raised the question with regard to the impact test. He stated that although when he first became connected with aeronautical work he had some doubt upon this, yet a closer connection with these tests had convinced him that it was of the greatest importance and his experience pointed to the connection which exists between low impact results and failures of cranks.

He called attention also not only to the light specific gravity but also to high conductivity of carefully selected light alloys for certain engine work.

He would be glad if Dr. Hatfield would state what was the effect of cold work on the properties of stainless steel and whether any value would be derived from this class of material for Rafwires.

Dr. W. ROSENHAIN, F.R.S. (National Physical Laboratory), said that he wished to congratulate Dr. Hatfield on his lecture, which was a species of brief text-book of the whole subject and which had been presented in a most clear and interesting manner. The greater part of what Dr. Hatfield had said was, of course, well known and generally accepted by scientific metallurgists, but it was perhaps desirable to emphasise this general agreement upon fundamental and essential matters because in the discussion on papers it generally happened that speakers devoted their attention to points upon which their opinions diverged—sometimes a little vigorously. The result was that those less familiar with the subject received the impression that scientific metallurgy was largely a tilting-ground for rival opinions and theories. Actually, this was very far from being the case; there always were—and he hoped there always would be—points at issue between different investigators; that was merely a sign of vigorous health in a rapidly-growing science. But what he desired to emphasise was that there was a very large body of well-ascertained facts and conclusions upon which they were all in substantial agreement and upon which practical men might rely quite securely. Much of the matter put before them that evening by Dr. Hatfield was of that nature.

Turning now to special points, he would deal with a few of the many which presented themselves—points of agreement as well as of disagreement. First, in regard to the question of silicon in steel. The difficulty in assessing the importance of a given silicon content as reported in a chemical analysis of any given steel lay in the fact that no distinction was drawn between the silicon which was present in solid solution in the steel—presumably as an iron-silicon compound—and that which was present in the more or less oxidised condition of various silicates. The latter were, of course, present in the form of non-metallic enclosures (sometimes called "slag enclosures"), and there could be no doubt at all that an undue quantity of silicon in that form was a very decided disadvantage to the steel—although opinion might still be divided as to precisely what was an "undue quantity." The silicon present in solid solution, on the other hand, was probably entirely harmless within the ordinary limits found in practice. This matter therefore resolved itself into a problem for the analyst, to devise a method for distinguishing between "dissolved" and oxidised silicon in steel. This was a problem certainly demanding research.

Turning to the question of pyrometry, he wished to draw further attention to the strict necessity of determining the temperature of the steel objects which were being treated. This was not by any means attained by merely placing a pyrometer at some convenient and supposedly representative point in a furnace and relying upon its readings to indicate the temperature of the steel. In the first place, in all industrial furnaces—and even in those specially arranged in the laboratory—there were usually enormous variations of temperature in different parts, and with slight changes of conditions—even with the mere ageing of the furnace and its concomitant effects on the material of the furnace walls and linings the temperature distribution in the furnace was liable to considerable changes. The indications of a single pyrometer at one point in a furnace could therefore serve only as a guide to the general working of the furnace. The temperature of a piece of steel heated in such a furnace would depend upon its position in the furnace and upon how long it had been there. Where large and important forgings were being heated it was surely worth while to apply a pyrometer directly to the forging itself. With proper precautions an optical pyrometer would furnish valuable indications, but wherever possible—and it would be found possible in most large forgings—a small hole should be drilled and a thermo-couple inserted. Some practical steelmakers might regard this as too elaborate, but the speaker could assure them that it was perfectly practicable and would pay for itself rapidly in diminishing failures. For smaller forgings, which had to be treated in numbers, the same procedure could not, of course, be followed, but for those cases he

wished to suggest the use of "dummies." This consisted in introducing into the furnace, as one of the charge, a "dummy" forging of the same material and dimensions as the forgings under treatment, but having drilled in it a hole for receiving a thermo-couple. The "dummy" would, of course, be spoiled by the presence of this hole, but a few such "dummies" would last for a long time. If now the "dummy" is charged at the same time as the rest of the small forgings and its rise of temperature is watched, this will serve as an excellent guide to the temperature of the forgings in the charge, particularly if from one charge to another the position of the "dummy" in the furnace is changed so that the behaviour of various regions of the furnace is kept under frequent observation. The "dummy" must, of course, be at the same temperature as the forgings forming the charge when they are introduced into the furnace. This procedure might also be regarded as a refinement by practical men, but again it was one which would lead to a very considerable advance in regularity of production. In actual practice the speaker believed that a very large proportion of so-called "mysterious" failures arose from faulty heat-treatment, and that this was due not so much to any want of knowledge of what the proper heat-treatment temperatures were, but to the fact that the real temperature of the steel was very different from what the operator believed it to be.

He would next like to refer to the question of welding, which had been mentioned by Dr. Hatfield, and he thought it was not going too far to say that in aeronautical construction, where heavy stresses had to be borne by joints and where failure would be so disastrous, welding should not be employed at all. He spoke thus strongly from actual experience of many failures of welded joints in aircraft. Nor was this surprising when the character of the welding process was considered. There was first the difficulty of securing a "sound" weld—*i.e.*, one free from actual cavities and from large enclosures of "dirt," generally oxide. There was no means of knowing whether a given weld was even approximately sound or not, and many were very unsound indeed. Then there was the fact that the internal structure of the steel was most adversely affected in the neighbourhood of the weld, where it always exhibited the coarse structure of seriously overheated or even burnt steel, and finally there was another danger-point which had not been as fully recognised as it should be. This occurred particularly in such objects as tubes and thin stampings at a little distance away from the weld itself. In tubes of ordinary size the distance was generally in the region of $\frac{3}{4}$in., and it was at that point that welded joints frequently broke both in practice and under test when the weld itself was fairly sound. This arose from the fact that at the time of welding we had a steep temperature gradient in the steel—it was heated to the melting-point at the weld itself and a few inches away it was cold; between these two points there were regions at all the immediate temperatures. Now in steel which was heated at a temperature just below the critical point—*i.e.*, in the neighbourhood of 650° to 700° C.—a change of structure took place with a degree of rapidity which varies with the nature of the steel, but which is generally very great in the soft steels used for welding. This change consists in the "balling up" of the pearlite—that laminated constituent which Dr. Hatfield's lantern slides had illustrated was converted into a structure in which the laminæ were converted into minute globules scattered through the ferrite. This change almost entirely destroyed the strengthening effect of the pearlite and left the steel in a very weak condition; it was very soft and ductile, but it had an excessively low elastic limit, and that meant a serious risk of failure under alternating stresses, particularly as this weakness was confined to a very narrow belt or zone, in which the resulting deformations would be concentrated. This was a point requiring very careful attention, and if welding were to be continued in spite of its obvious and serious risks, it should be insisted upon that this particular evil effect—and incidentally also the overheated structure close to the weld—should be removed by heat-treating the weld after its completion, as by normalising it. This was not possible in many structural joints, but that

circumstance merely served as an additional argument against the use of welding for such joints.

Finally he (Dr. Rosenhain) would like to refer to two questions relating to mechanical testing which had been raised by Dr. Hatfield. The first was in connection with the determination of the "true" elastic limit as distinct from the yield-point. It was the custom to speak of this as impracticable in commercial testing, because it involved the use of a delicate instrument called an "extensometer." This was really a bugbear—the instrument in question was not more delicate or difficult to use than many others now regularly used in commercial practice. He would illustrate this point by a reference to a subject with which many of those present would be fully familiar, viz., the use of accurate gauges. What had once been regarded—implicitly, if not avowedly—as too delicate and difficult for commercial practice had now become an everyday matter. Similarly, testing with a simple form of extensometer, delicate enough to detect the elastic limit, could be made perfectly possible as an everyday matter. It certainly would require some "tuning up" of works testing practice, and might possibly even entail that calamity—the employment of a rather better-paid and skilled man to do this vitally important work of testing—but that would all be to the good, and he felt certain that the steelmaker as well as the user would benefit enormously by such a development. The lesson that the rough-and-ready, apparently quickest and cheapest, "good enough for works practice" methods were not really the most economical and profitable would certainly be learnt in this case sooner or later. He hoped that those concerned would learn it soon, and voluntarily, without waiting for the pressure of necessity to force advance upon them, as had unfortunately happened in so many cases.

Perhaps the most serious point of divergence from the views expressed in the lecture concerned the impact test. Dr. Hatfield had, of course, referred to the Izod test, and that was a form of impact test which was, in the speaker's opinion, open to serious criticism as compared with the Charpy form of impact test. The most serious point of that criticism, but one which he regarded as vital, was that in the case of Izod tests of ductile materials the test-pieces were not broken, but merely bent over. In the Charpy machine it was very rare to find a piece which was not completely broken. The main point was that he wished to testify to the very great value of a properly-conducted impact test. In a long series of investigations of failures in steels of all kinds the impact test had been found most invaluable. There were numerous cases of "mysterious" failure, as was well known to many present, in which tensile test failed to show any defect in the material; in nearly every instance, however, where this occurred the impact test at once showed the defective character of the material. He had yet to find a case in which the close correlation between micro-structure and impact figure failed to hold. No doubt examples could be quoted which constituted apparent exceptions to such a generalisation, and since no one had applied the test systematically to every possible grade of steel it would not be surprising that in some special materials cases might be met where it was difficult to recognise the details of micro-structure which were associated with low impact figures. Such exceptions, however, merely emphasised the general fact that in the vast majority of cases a "bad" micro-structure enabled one to predict a bad impact figure with entire certainty. The border-line cases, where it became a delicate question of judgment whether the structure could be called "bad" or not, might present some difficulty, but for practical purposes there was no doubt that the micro-structure could be used with entire reliability to eliminate at sight the really dangerous cases of grossly wrong heat-treatment. It was for that purpose that he had recently, in that room, put forward the suggestion that some reference to micro-structure should be incorporated in specifications. There was sure to be some opposition to that suggestion, but that applied to every forward step in such matters. The impact test itself was in the same position, but its utility was being proved daily both in the laboratory and in the practice of aircraft inspection, so

that it would have to be regarded as a permanent and valuable addition to our testing equipment. There was, of course, the consideration that an increasing number of testing methods implied a tendency towards undue complication in our specifications and tests, and that would have to be avoided as far as possible by a wise selection and distribution of tests. On the other hand, our requirements were steadily increasing in stringency—nowhere more so than in aircraft—and our materials were also of increasing complexity of composition and delicacy of treatment, so that increasingly stringent methods of testing and supervision would have to be faced, both by the manufacturer and by the user. It was essentially a matter of cost—a question of the price we should have to pay in order to meet with certainty and safety these increasingly stringent requirements.

Mr. A. A. REMINGTON: I have read Dr. Hatfield's paper with the greatest interest, and, with the author, would like to point to the necessity for co-operation between the engineer and the metallurgist. Both are dealing with structures, and their work is most intimately connected. The structure of the metallurgist is of the microscopic order, whilst the engineer uses the structures provided by the metallurgist as a basis, and from it constructs structures of the terrestrial order.

To get the best results it is necessary for both to study and understand to the utmost possible extent their own product, but furthermore also conjointly study the combined product so that perfection in the combination may more nearly result.

The paper under discussion is written from the metallurgist's point of view, and throughout the early part of it there is an undercurrent of criticism of the engineer for lack of knowledge of his structures.

While in general terms I am in agreement with the author as to the elementary state of knowledge on the part of the engineer in regard to the causes that affect the behaviour of his structures, I think I may safely say that the leading engineers know at least as much on their side of the subject as the leading metallurgists do theirs.

A tendency of human nature that has existed throughout all ages is to hide ignorance behind a mask of mysticism, and I am afraid that in the past this tendency has influenced the relations between engineers and metallurgists. We have to-night one out of many recent instances of metallurgists throwing this aside, and I would strongly recommend engineers to follow suit.

To get proper sympathy between engineers and metallurgists there is a great necessity for education, and nothing has struck me so forcibly as the appalling ignorance of the more recent practical developments of metallurgy as supplied to engineering on the part of many engineers, whom one would expect to know better.

I am glad to say that the old gulf which appeared to exist between engineers and metallurgists until recently, is now practically non-existent thanks principally to the powerful forces brought into play by the war, and I notice a new type of scientific metallurgo-engineer is being developed. A more rapid development of engineers of this type, specialised in the various branches of engineering, would do much to wipe out the existing rule-of-thumb methods that persist in many engineering works even where research has pointed out the scientific method.

Scientists constantly plead for more science to be applied to our industries; such appointments accompanied by suitable powers would materially help to bring this about.

Dealing with design from the engineer's point of view, I would, without going into details, point out that these abominations of the metallurgist, "sharp corners, shoulders and fillets of small radius," may even be advantageous, just as an eminent metallurgist recently told a representative body of engineers in regard to ingot defects, that "the same kind of ingot defect may be at one time

fatal and at another time actually beneficial," and " . . . if we are asked whether slag occlusions are deleterious, and we inquire how they lie in relation to the direction of stress, our answer will not always be in the affirmative." (Mr. H. Brearley, in a paper before the Manchester Association of Engineers, March 24th, 1917.)

As an example of the advantages of small fillets one might instance the aero crankshaft. Such a shaft is governed in length by the sum of journal and crank pin lengths, web thickness and fillets. The usual four-throw crank comprises 5 journals, 4 crank pins, 8 webs, and 16 fillets; the fillets being waste length of crankshaft and therefore of engine as far as bearing surfaces are concerned.

Assuming $\frac{3}{8}$in. fillets, they account for 16 by $\frac{3}{8}$in. = 6in. of engine length, and therefore a reduction to $\frac{1}{8}$in. would save 4in. of length. It would therefore appear advantageous to adopt a larger diameter lower stressed crankshaft with small fillets and thereby produce a shorter and obviously lighter engine.

Now for sharp corners.

I instanced in discussing a paper before the Institute of Automobile Engineers a crankshaft that invariably broke, and a sawcut prevented the breakages by giving more length for local deflection, thereby reducing the localisation of stress, but at the same time introducing a sharp corner, which while not essentially desirable, was workable and less detrimental than the sudden change of section it eliminated. The following sketches are explanatory.

In regard to rational testing procedure, in my opinion there is much room for co-operation.

Routine testing as representing unproductive work should be reduced to the minimum providing sufficient safeguards for suitability of production are retained.

The usual testing procedure which concerns itself with the behaviour of a material after its yield point, or perhaps its true elastic limit is passed, is of little interest to the engineer as affecting the conditions under which materials are used in engineering structures, and tests which deal with the behaviour of materials under conditions of stress beyond the yield point would appear to be only useful in so far as they prove the standard of quality of a material and its suitability for the agreed working conditions.

Study of the behaviour of materials in these abnormal conditions is much easier and simpler than those under the useful conditions, and unfortunately appears to have attracted far more attention. I should like to join with the author in advocating the great advantages of more research in regard to the behaviour of the materials comprising structures under actual working conditions.

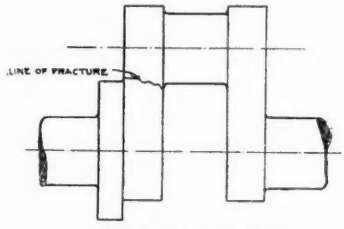
I agree with much the author says regarding " factors of safety," " factor of contingency," or what it sometimes amounts to, " factor of ignorance," but would suggest that more prominence be given to the effect on the permissible stress of " modulus of elasticity," which involves considerations of rigidity, or permissible distortion of the parts comprising the structure.

The fact that the " modulus of elasticity " is a " fundamental " property of a material, and therefore substantially unalterable, which means that it is the same for 100 tons of steel as for the mildest steel, has a profound, although not fully recognised, effect in limiting the extent to which engineers can make use of those wonderful products of the metallurgist's art in weight reduction.

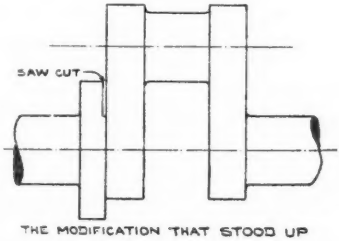
Another point which I fear may not have been brought into sufficient prominence by the author is the question of stress reversal, and I would particularly call attention to this in connection with the consideration of aluminium as a useful alternative to steel. The illustration given might lead one to suppose that aluminium could usefully supplant steel for many parts, but such a conclusion is

not justified and the illustration must be considered merely as such and in a very limited sense.

I should like to ask the author what is his interpretation of the word "soft," as applied to the case of case hardening steel, also for a more complete definition



THE SHAFT THAT BROKE



THE MODIFICATION THAT STOOD UP

of the meaning applied to the word "shock" as applied to extreme cases of acceleration, so as to be quite sure we, *i.e.*, metallurgists and engineers, both mean the same when we use these words.

In conclusion, I should like to congratulate Dr. Hatfield and your Society on one of the most interesting papers I have ever had the pleasure of discussing.

Mr. J. H. S. DICKENSON: I have to congratulate Dr. Hatfield on an excellent piece of work which should be of considerable use to all engineers concerned with the use of aeronautical steels. At the same time I feel that the ambitious attempt to cover so wide a field in the limits of a single paper has the great objection that many of Dr. Hatfield's statements are unproved, and that insufficient opportunity is given for discussion of the numerous statements of merely personal opinion. Amongst many of the useful points mentioned by Dr. Hatfield, I am glad to see further reference made to the importance of rigidity in crank cases, etc., the outstanding value of a high elastic limit in many cases, and the bad effect of sharp corners. The recurrent neglect of such matters, repeatedly insisted on in the past, is really surprising. There are, however, some statements made by Dr. Hatfield which it is almost one's duty to challenge. Amongst these I place the references made to Bend Tests and to Notched Impact Tests.

It is, perhaps, only fair to state that some two or three years ago I should have been more in agreement with Dr. Hatfield in this connection than I am to-day, but time has brought conversion. Dr. Hatfield says that the simple bend test "is not appreciated and imperfectly understood." Also that "he is convinced that too much reliance is often placed on notched impact tests and a much too ready interpretation placed on the results." Because, in his opinion, the latter testing method is liable to abuse he would leave the whole question in the hands of "unbiassed investigators" and so put back the metallurgical clock. With regard to the simple unnotched bend test I can only say that if there is any lack of appreciation it is not because of lack of use in the past, or due to prejudice against innovations. This method of testing, useful no doubt in the case of, shall we say, steel and malleable iron castings, breaks down badly when applied to certain alloy steels.

It is a fact which, so far as I know, will not be denied by anybody who has taken the trouble to put the question to the proof, that the notched impact test is capable of showing differences in physical properties of steels which are not revealed by the tensile test, by ordinary bend tests or, and this is most important, by micro examination. This is most particularly true of certain nickel chromium steels. The information required is given by any of the notched impact test methods with which I am acquainted. The Charpy, Izod, Fremont, etc., all give

indications which may differ somewhat in character, but point in the same direction, *i.e.*, determining whether the steel tends towards fragility or non-fragility. In the hands of a competent observer it is possible to deduce much the same thing from a small test bar suitably notched and broken in a vice with a fitter's hammer when the manner of breaking and the appearance of the fracture give quite valuable results. Now it is begging the question for Dr. Hatfield to say that examples could be quoted from practice of material which has showed low values, but which worked satisfactorily for long periods under satisfactory conditions. The perfectly true answer which can be made to this is, that fractures have occurred; I do not propose to quote specific cases in brittle nickel chromium steel which would not have occurred had different heat treatment produced a better physical condition, easily ascertainable by the impact test, but not to be determined by either the tensile test or by micro examination. I am quite aware that certain investigators claim to be able to distinguish microscopically between brittle and non-brittle nickel chromium steels. I consider this to be a very rash assertion which would probably break down in practice. To prove this I have brought to-night a few lantern slides which may be of interest in this connection. No one denies the ability of the microscope to distinguish in many cases between correct heat treatment and heat treatment which may lead to disaster. For example, this slide shows the satisfactory micro-structure associated with good mechanical properties—in this case high elastic limit—which was found at one end of a forging which failed by fatigue. The next slide shows, at the same magnification, *i.e.*, 250 diams., the structure found near the point of fracture. This proves conclusively that faulty heat treatment has permitted the separation of free ferrite, and thus to an unduly low elastic limit with increased liability to failure by fatigue. A more spectacular case could be provided by showing how good and bad methods of annealing may produce large and small grain in steel forgings. Such cases could be supplied wholesale, and prove the many possibilities of systematic inspection by microscopic methods. Now, however, I show a micro-section at 250 diameters of a sample of nickel chromium steel in the oil-hardened and tempered condition. This specimen gave a yield point at 42.0 tons sq. in., maximum stress at 55.0 tons sq. in., and an excellent Izod test of 44 ft. lbs. The micro-structure is troosto martensite showing dark etching acicular bundles with no free ferrite. The next slide is the micro-structure, at the same magnification, of another bar of the same lot of steel, but differently heat treated. This specimen gave an almost identical tensile test result, yield, maximum stress, elong. per cent. and reduction of area per cent. all being nearly the same, but the impact test is far lower, at only 16 ft. lbs., and the steel may now be described as brittle. The two micro-sections are, however, as much alike as two structures could be, this slide showing the same acicular arrangement in troosto martensite with no free ferrite, the individual crystalline grains being of similar size. The only difference is that in this case, under certain conditions of fracture, rupture will run rapidly along the crystal junction whereas in the previous case the amorphous cement, or whatever Dr. Hatfield would choose to call this interesting intercrystalline bond, would not allow of this. I may next show another pair of slides showing similar micro-structure to one another, but different to the pair first shown which give, as before, with exactly similar tensile test results, great differences in the Izod test, proving considerable toughness in one case, and decided fragility in the other. I do not think that any real distinction can be made between these two micro-structures, and consider that here again we must assume that the only difference lies at the crystal junctions and is connected, in some way, with the so-called amorphous cement.

Dr. Hatfield will, I know, because he has looked for it, confirm me in saying that this lies outside the reach of the microscope. Now I can supply many such comparisons to prove that to place the acceptance of nickel chromium steels in dependence on their micro-structures would be more likely to lead to the condemnation of much good steel than the elimination of bad material. The value of the

impact test, in such cases, does not admit of further questioning. The main objection to the impact test, on the part of steel makers, is based less on the causes of variation mentioned by Dr. Hatfield, than on the great difficulty of achieving standardisation, and on the practical difficulty of milling many small square test pieces in these busy times. In the latter connection the cylindrical impact test, now being worked out by Captain Philpot, is highly promising. As regards standardisation, I do not consider that it matters much which method is adopted so long as general agreement can be arrived at. The Izod impact test method, which is theoretically inferior in some ways to the Charpy, has given excellent results in practice and is in such general use that its immediate standardisation, preferably on the basis of a round test piece, seems to me highly desirable. However, the Engineering Standards Sub-Committee on notched bars has this matter in hand.

I will occupy little time in discussing further points, though there are many which I should like to raise. I notice that Dr. Hatfield, on page 291, when discussing forgings and drop stampings, refers to the method of manufacture of aero crankshafts by which a bloom is first forged to crank shape and then finished between dies. I have been personally directly interested in this process, and I know that it has been highly successful in practice. I have, on two previous occasions, in this same room, shown the advantages which the method presents, and it should not be necessary for me to do so again to-night. The advantages of this process of crank manufacture, as compared with others which I have had the opportunity of examining (and we have ourselves made crankshafts by several different methods) are exceedingly well marked. This is well known to those engineers who have had the opportunity of comparing various kinds of crankshafts. Dr. Hatfield objects that great care is required in producing such crankshafts. I do not know that Dr. Hatfield has ever seen a crankshaft made in this way, but in any case the answer is, that such care is taken.

I am pleased to read Dr. Hatfield's remarks on the advantages of case-hardened gear wheels, with which I am generally in agreement. I notice that Dr. Hatfield views the relation of hard skin to soft core in the way in which it presents itself to me, his explanation of the behaviour of case-hardened parts under stress coinciding with opinions which I have previously expressed.

In conclusion I would like to add that the needs first of all of the automobile industry, and later of the aero constructor, have reacted in a most beneficial way upon the steel industry. These demands have undoubtedly led to metallurgical progress, and by requiring higher physical properties, and greater uniformity in the products of steel works have greatly accelerated the application of scientific method in works practice, previously only found to any conspicuous degree in armament works. This will no doubt have an effect, in turn, upon metallurgical products of a somewhat different order, that is to say, railway material and structural steels. For this reason the efforts of the aero designer to improve his machine will have a much more far-reaching effect than might at first glance be expected. I have again to thank Dr. Hatfield for a most interesting paper and only wish that time permitted to deal with more of the points he has raised.

Sir CHARLES PARSONS, F.R.S. (Chairman), in winding up the discussion, said that he considered that engineers realised the great advances which had taken place in the metallurgy of steel both as regards production and mechanical properties. He thought the engineering profession generally was now taking a much more intimate interest in steel, and this was really necessary if the excellent results which were now possible were to be satisfactorily and usefully obtained. He quite agreed with the lecturer that sharp corners, causing, as they do, local concentration of stress, should be eliminated wherever possible. With regard to the use of aluminium which had been mentioned by several speakers, he thought that the high thermal conductivity of that material would greatly assist its more extended use in internal combustion engines. He was much interested in the

hardening of the piston head mentioned by the lecturer and agreed with the explanation given.

The question of impact testing seemed to have taken a predominant place in the discussion, and he would like to ask the lecturer how manganese steel in the toughened condition responded to this form of test.

He had much pleasure in proposing a vote of thanks to Dr. Hatfield for his lecture.

BY CORRESPONDENCE.

Mr. A. E. BERRIMAN: 1. I consider that the Aëronautical Society is to be congratulated on having had such a comprehensive paper on a subject of so much importance to the aircraft engineer. To its value I can add nothing beyond the emphasis of those aspects of Dr. Hatfield's subject that appear to me more especially significant. Thus, in the first place, I would desire to underline the direct connection between metallurgy and the problems of design.

2. The modern science of physical metallurgy, which has figured so prominently in Dr. Hatfield's remarks, aims at investigating what is commonly known as the quality of material, and it is this same quality factor that looms so large in the theory of weight saving. Economy of material is an object towards which all branches of engineering make a common cause, but it is the very axis of aeronautical technology.

3. To take the case in point, many members are no doubt familiar with Mr. Lanchester's well-known paper on the theory of weight-saving in engine design, which was read before the Institution of Automobile Engineers in 1906. In that paper it was pointed out that the weight per horse-power of an engine varies as the linear dimension divided by the 1.5 power of the specific strength of the material. By specific strength is meant the ratio of the safe stress to the density. For example, if the density of steel is three times the density of aluminium, then their specific strengths will be equal when the ratio of their safe stresses is 3.1. This is another way of saying that steel exceeding about 35 tons tensile is lighter than aluminium, when the exigencies of design permit the part to be made to the appropriate dimension. Extraneous considerations frequently govern the choice of material in practice, but the academic aspect of the case is none the less worthy of attention.

4. The competition between alloys of iron and alloys of aluminium is becoming very keen. As time goes on it will become more acute, and it is in this field of physical metallurgy, which Dr. Hatfield has so attractively discussed, that the engineer will find most aid to a clear comprehension of the pros and cons of the subject. The possession of allotropic modifications that are usually within reach of temperature changes within a range of the solid state gives iron a unique responsiveness to heat-treatment, and confers upon its alloys an advantage over those of most other metals. But this very fact at once throws upon the shoulders of the engineer some of the metallurgist's responsibility for the quality of material.

5. Ideally, every kind of steel should be heat-treated. In practice it should at least be a rule to heat-treat alloy steels. Their use in the raw state is both dangerous and waste of money. This may be a debatable statement in respect to some special forms where strain-hardening is an important factor, but in respect to stampings and forgings it should be regarded as axiomatic. It is because of the magnitude and importance of the divided incidence of this responsibility that I make a plea for greater simplification in the varieties of steel.

6. Dr. Hatfield is one of those who worked so hard on the Steel Committee of the Institution of Automobile Engineers, which drew up the specifications that

have recently been published by the Engineering Standards Committee. I commend that document (Report No. 75) to the attention of those who have not yet seen it. My own feeling is that we do not so much need new steels as more knowledge of those we possess, and I consider this publication of the E.S.C., coupled with the research now proceeding on samples of steels in question, to be an important and, I hope, far-reaching step in this direction.

7. Particularly do I consider that the more widespread use of comparatively few types of steel would do much to enlarge the practical value of micrography. It is true that expert metallurgists disagree as to the visibility of the more abstruse defects in steel, but the fact remains that this process is an invaluable aid to our understanding of structure, and for my own part I have faith in the microscope's ultimate elucidation of present obscurities. It is not by the study of one, but by the comparison of hundreds of these steel pictures that this result will be attained, which is why I emphasise the importance of concentrating our attention as much as possible on the qualities of relatively few types of steel. To do so in the main is no deterrent to progressive experiment.

8. Dr. Hatfield has referred to many interesting special steels. None is more striking in its composition and character than high chromium steel, as developed by Mr. Brearley in the laboratory over which Dr. Hatfield now presides. Some time ago Mr. Brearley sent me some of this steel for trial, and it seemed to me that it possessed qualities that rendered its use for high-temperature valves very promising. I expect many of you have already observed the word "stainless" stamped on the more recently purchased cutlery.

9. I am afraid I have been rather discursive, but so, too, was the author of the paper, and the points I have tried to underline are, perhaps, somewhat scattered in his text. If, however, I have emphasised the immediate character of the direct connection between metallurgy and design, the importance of quality in the theory of weight saving and its significant influence on the competition between steel and aluminium, I shall have made my first points clear. If, further, I have succeeded in interesting engineering manufacturers in the desirability of trying to use standard steels, and the metallurgists in the bearing that this may have on our micrographic knowledge of steel structures, I shall have advanced a matter that I have much at heart.

Professor DALBY: This paper is so comprehensive that it may almost be regarded as a treatise, and it is not possible, therefore, to discuss it at all adequately in a brief statement. It raises many points of interest, and contains information of the highest value which should have the effect of introducing more scientific methods of testing amongst engineers, certainly amongst those who are users of steel.

I will confine my remarks to one or two points. Dr. Hatfield mentions that "the 'maximum stress,' as generally understood, *i.e.*, $\frac{\text{maximum load,}}{\text{original sectional area,}}$ can as a rule be determined exactly." He then states that "what the maximum stress signifies may not always be the maximum stress of which the material is capable. It has been shown that it is possible with ductile material, by a simple modification of the shape of the test-piece, to make it withstand a higher stress than the maximum as ordinarily determined." I can illustrate this from some results which I have published elsewhere.

In testing a series of test-pieces from the same material, of continually diminishing length, it is found that as the length diminishes a point is reached where the maximum stress, as defined above, begins to rise until the length has been reduced to $\frac{1}{8}$in., when the stress, normally 30 tons per square inch, appears to be nearly 50 tons per square inch.

Dr. Hatfield also says that "it would be an interesting addition to the data usually obtained from the tensile test if the actual breaking load per square inch of reduced cross-section could be more frequently determined." In the tests carried out in the Engineering Laboratory of the City & Guilds (Engineering) College this stress is generally determined, and it is calculated by taking the load on the specimen at the instant of fracture (which is recorded on the diagram) and dividing it by the actual area of the bar at fracture.

The stresses measured in this way show a much narrower range of variation than the range for the corresponding ultimate stresses. I quite agree with Dr. Hatfield that it is a measurement which should always be taken when possible; but I do not quite agree that "the percentage reduction of area is a better guide to ductility than elongation, and has the further advantage of being less dependent on the length of the test-piece." I have found considerable variations in this quantity due probably to local variations of quality which have little effect on the elongation taken on longer lengths; I should not rely on a series of measurements of contraction of area as an index to ductility apart from the elongation test.

I note that Dr. Hatfield, although he does not mention it specifically, regards it as important that the test bars should be geometrically similar. This is fundamental if any method of testing is to be generally adopted which shall furnish comparative figures of ductility. The present haphazard way of defining ductility by reference to a certain length without regard to the shape or diameter of the specimens gives figures which are not comparable one with another even for bars of the same material. I am convinced that a great deal of the uncertainty in the ductility tests could be avoided if it were possible to agree to a standard form of test-piece, which need not necessarily be long. I have shown elsewhere that reliable tests can be made on short specimens by measuring the elongation between shoulders, which, of course, includes the end effects. If the test-pieces are geometrically similar, the effect of the ends can be included equally with the effect of local contraction.

The problem now is to get a general agreement among users of material as to the form of test-piece which should be adopted, so that it may be practically possible to prepare geometrically similar and comparatively small test-pieces at little cost, in order to include a large range of material within the comparative limits.

Mr. R. T. ROLFE: Dr. Hatfield's extremely lucid and comprehensive treatment of his subject is such that very little adverse criticism is possible. One or two minor points, however, are worth further consideration, one of them concerning the Izod and Charpy impact tests.

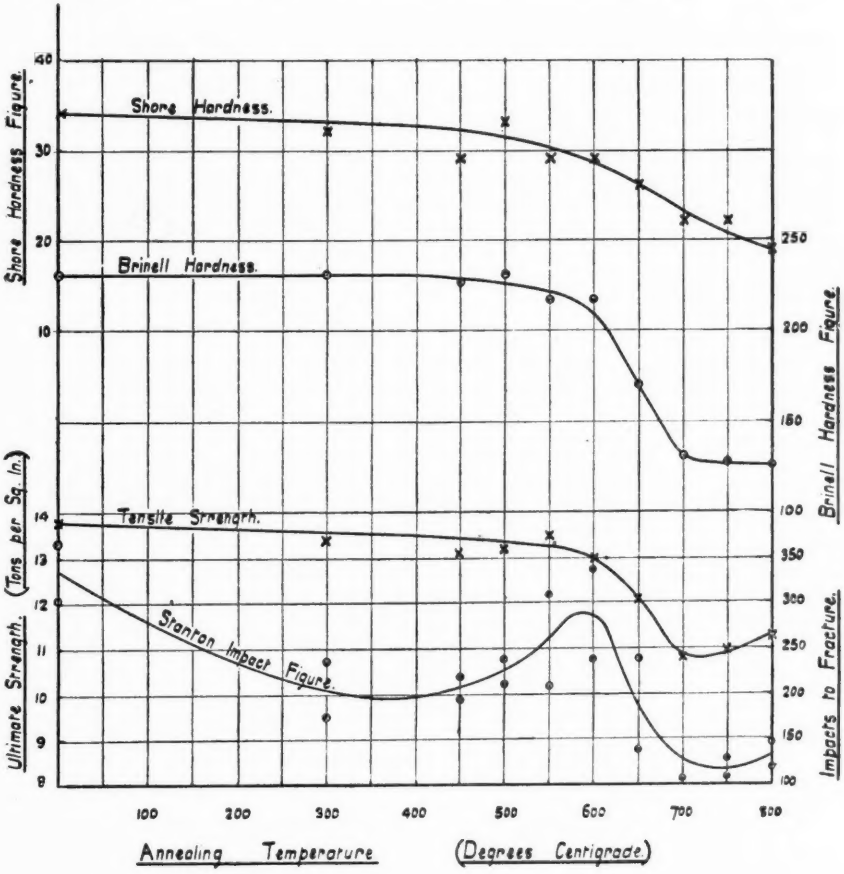
With reference to these, Dr. Hatfield states that "the variability in the results for the same material, for no *apparent* cause, forms, in my opinion, a serious objection to the test." It has, however, been previously pointed out that in the case of these tests, the variation in the results from the same test bar is practically always much less than the variation between good and bad samples of the same steel. As an instance of this may be quoted the following results, which have just been obtained by the writer from different three-notch test bars of the same nickel-chrome steel, heat-treated in different ways:—

1	2	3	4
3·5	13·5	19	24
3·5	13	20	24
3·0	13·5	19.8	27·5

The figures given below illustrate about the maximum variation that may be obtained from the three different notches of three test bars of another nickel-chrome steel, in a state supposed to conform to R.A.F. 2a (now 2b), 40 foot lbs. being demanded for the Izod test:—

A	B	C
18.5	26	41
21	27	42
14	16.5	35

Here, and as is usually the case, the variation in impact figure does not affect the ultimate issue, which is whether the steel conforms or does not conform reasonably with the specified figure. It is obvious that, considering impact results only,



A and B are much below specification, while C would be accepted. The comparatively small variation in the three results from the same notched bar with the nickel-chrome steels is well demonstrated in Dr. Hatfield's table at the top of page 294.

Further investigation is, of course, necessary to endeavour to ascertain the cause of the present variation, and, if possible (although this is doubtful) to eliminate it. In this direction the writer is now doing a certain amount of work.

It is interesting to read Dr. Hatfield's remarks on the Shore and Brinell hardness tests, and the relation between these two tests and the tensile figures of a material. Without going into the theoretical reasons for and against the

existence of these relationships, which matter is very fully dealt with in the recent report of the Hardness Tests Research Committee of the Institution of Mechanical Engineers and the discussion thereon, there is undoubtedly in certain cases a fairly well marked relationship between these properties. In this connection it may be interesting to refer to the accompanying figure showing the effects of annealing cast iron at different temperatures, the results having recently been obtained by the writer in connection with a question of the annealing of some C.I. pistons. It will be seen that this is a case where the two types of hardness test not only agree fairly well with each other and with the tensile test, but also agree to some extent with the results from the Stanton impact test, obtained by using a ½in. fall of tup and making 80-90 impacts per minute. The lowered impact resistance between 300° and 500° C. appears to be connected with the pronounced brittleness to impact of nearly all steels when the test is actually made at these temperatures (compare the increased brittleness of mild steel when worked at the blue temperature, than when worked cold). A lowered impact resistance is also noticeable, but to a very much less degree, with *some steels*, tested cold, but which have previously been tempered at about the same range, 300-500° C.

It appears, therefore, that the somewhat maligned Stanton test, which I am glad to note Dr. Hatfield considers "should offer useful data to the engineer" may in certain cases be worth trying. With cast iron, Charpy and Izod tests give a value of about 1 foot lb., either in the material as cast, or annealed at any temperature, and therefore do not discriminate between the material treated in different ways. From the work already done, it appears that there may be some relation between the Stanton and some of the other mechanical tests employed.

As to the precise value of the Stanton test, the writer makes no claim. He is only concerned to show that it is worthy of some attention. Further work is in progress on the annealing of C.I., with intent to verify (if possible) the results of the work already carried out.

Throughout Dr. Hatfield's exceedingly valuable paper, many suggestive points are raised, the discussion of which in the course of a short communication is impossible, but which should stimulate in many new directions the future progress of all engaged on aero constructional work.

Mr. ARTHUR STUBBS: On page 257 the author refers to the engineer responsible for producing aero parts having a sound appreciation of the metallurgical side of the business, and I would like to couple with that a knowledge of drop forgings, as it is quite evident from a number of the designs I have had to criticise from time to time that a greater knowledge on the part of the engineer of the art of the drop forger, and the limitations of the process, would result in more satisfactory and reliable parts being obtained.

On page 260 the author, when commenting upon the factor of strength, refers to the difficulty of measuring the true elastic limit, and I, personally, do not see any real or sound reason why the elastic limit should not be more readily and frequently obtained in works practice. A step in the right direction would be, when framing specifications, to state how this property shall be obtained, and the clause inserted in specifications adopted by the American Society for Testing Materials is worthy of consideration by English engineers.

I fully substantiate the author's remarks upon the same page, when he refers to the necessity of paying more attention to the consideration of internal stresses left after forging, and also after heat-treatment, and when discussing the factor of safety more consideration should be given to quantitative destruction tests.

On page 263 the author refers to the relative stiffness of connecting rods of H section and hollow tubular section, and while it may be true that the H section appears to be superior in preventing buckling, I think it may be found in actual tests that, due to the more satisfactory forging one can make when of the tubular

section and the better state of the material due to less internal stresses, the tubular section is the better for many purposes.

On the same page, when dealing with mechanical testing, the author refers to a well-known fact that for many steels the yield-point is not of a definite value, but if a tensile test is carried out with the aid of an extensometer, as it can be even in works practice, in the hands of an experienced inspector, the true elastic limit, which is really what is required, can be determined within a reasonable degree of accuracy.

I quite agree with the author that the percentage reduction of area is a better index to ductility than percentage elongation.

On page 265 the author refers to impact tests, and I do not agree with the conclusion he arrives at when he states that in detecting brittleness of a dangerous nature the general usefulness of this test remains to be demonstrated. It is within my experience that one may find materials giving the same result in tensile test yet differing very greatly under the impact test, and there is one important point which I would like to ask information on while dealing with this matter, and that is, the reason why, of two nickel-chrome steels of the same analyses, but different casts, used in the same forgings, and given identically the same treatment, one will give a good impact figure and the other a very bad one? While agreeing that these tests are not as satisfactory as one would like to see them, one must remember that they have only recently been adopted in this country, and the only machine that has had any great use has been of the Izod type, which machine is open to many serious objections. There is no doubt that with the Charpy type of machine more regular and uniform tests can be obtained. Furthermore, if one makes a series of tests upon unnoticed test-bars, it will be found that the values yielded are, as might be expected, more uniform. Further research upon this form of test is urgently needed, but there is no doubt in my mind that the impact test has come to stay.

When referring to Brinell tests on page 267 the author refers to this test as being a not infallible guide as to machining properties, and I would go further and state that this test is never an indication of the machining properties of metals.

I would like to put in a plea for the more extended use of the torsion test, as referred to on page 268, as so many parts have to withstand torsional stresses, and it is well known that certain alloy steels in this respect show up distinctly better qualities than others.

On the following page the author deals with the steels used in aero construction, and states that some people successfully employ chrome vanadium steels, and if this is the case it would appear desirable to know something more about it, but the author makes no further reference to this material in any part of the paper.

Where the total values of various steels are being considered I think it will be found that this material exhibits most useful properties, some of which are not given by other alloy steels.

On page 286 the author refers to the use of air-hardening steels, and predicts that when they are more completely understood they will be used to a much greater extent than at present. Unfortunately the employment of this steel for drop forgings has not given universal satisfaction. This is due mainly to two causes: (1) The specifying of this material for a design which cannot be conformed to without spoiling the material, and its use for parts where a steel of less tensile strength would be equally and even more satisfactory. (2) The general lack of knowledge on the part of drop forgers as to the properties of this material, and the handling of same by men who have had no experience in forging steels of this character. Any part necessary to be made in an air-hardening steel as a drop

forging should be of simple form; sharp angles, deep ribs, and thin webs should be things unheard of and unseen in the design.

On page 287 the author refers to the use of stainless steels for aero valves instead of high-speed steels, and it is much to be regretted that (especially in the early days of the tungsten shortage) its use was not more encouraged. It is within my knowledge that stainless steel has given equal and in some cases better results than high-speed steel, and it also has the advantage of being much cheaper.

On page 291 the author refers to the use of alloy steel for forging and drop stamping, and I heartily agree with him that all alloy steels should be heated up slowly and uniformly, as a good many of the failures and also rejections of drop forgings have been due to not carrying out this simple practice. Insufficient attention has been paid also to methods of forging, especially as regards the preliminary work of forming or roughing out prior to stamping. The author also states that there are certain steels which probably the drop stamper would be wise to avoid, and if this is the case it is highly desirable that a recommendation from such an experienced metallurgist as the author should be forthcoming.

*I have previously advocated the closer co-operation of the designer, steel-maker, and drop forger, and I am glad to see the author so strongly recommending what should appear really obvious. It is not so long ago that the steel-makers, when requested by the drop forger to give the analysis of the material he was supplying to a certain specification, absolutely declined to give this elementary information, but the issuing of the Engineering Standards Committee's Specification for Automobile Steels has done much to break down the barrier which existed between the two parties.

No emphasis is needed upon the point raised as to the evil effects of overheating a drop forging, and the same remark may be extended to include cold working, particularly steels of an air-hardening character.

On pages 294-5 the author gives exceedingly useful information as regards the physical tests obtained from three steels after various heat-treatments, and it would appear desirable to have the analysis of the three respective steels.

I would like to thank the author for the large amount of ground covered and information given within the compass of a single paper, which I am sure will be of much interest and service to the aeronautical industry.

Dr. THOMPSON (Sheffield University): Dr. Hatfield's most interesting contribution to our knowledge of special steels will be received with great pleasure by metallurgists and engineers alike. Especially valuable are the test results on page 294, correlating, as they do, the ordinary tensile properties with impact values.

There are very many points which invite discussion, but the one to which attention is perhaps most worth drawing is Professor Arnold's alternating-stress testing machine. It is mentioned that resistance of steel to this form of alternating stress usually decreases as the maximum stress increases. Over a very broad range of carbon contents this is undoubtedly true, but a consideration of all the published data revealed the extremely important fact that for a large variety of special steels, when in their normal and annealed condition, the alternations endured reached a fairly pronounced maximum in the neighbourhood of 0.3 per cent. carbon. The following figures show this for a 3 per cent. nickel steel, but tungsten and chromium steels give analogous results:—

ALTERNATING STRESS (ARNOLD) TESTS FOR 3 PER CENT. NICKEL STEEL (NORMALISED).

Carbon, per cent. ...	0.12	0.28	0.47	0.67	0.91
Alternations	319	350	278	209	149

*See the I.A.E. Proceedings, 1915.

It is a curious fact that in the quenched and tempered condition this maximum at 0.3 per cent. carbon no longer appears, and Dr. Hatfield's remarks are completely verified.

It may be worth noting that the Arnold test gives results which are reflections of no single item of the tensile test—unlike the Wohler test, which merely reflects the yield-point. Thus for a 70-ton maximum stress alternations have been registered from 90 to 300, while steels which all gave an elongation of 15 per cent. have withstood from 50 to 200 alternations. It is therefore apparent that this form of alternating stress test does reveal some property not brought out by tests in common use.

One point in the paper was of special interest, viz., that which dealt with the true elastic limit. For several years the writer has given considerable attention to this point. The actual determination of the *true* elastic limit as distinct from the yield-point, from which it differs sometimes very greatly, especially in annealed steels, cannot be carried out as a routine method of practical testing. The writer has, however, shown that the coarseness of the micro-structure of the steel is responsible for the low elastic limit in annealed steels, and a measure of the limit in most steels of the same composition, and some use might conceivably be made of this point in routine work. To those interested in this subject reference may be made to Professor Arnold's classical paper on "The Factor of Safety in Marine Engineering," published in the "Trans. Institute Naval Architects for 1908," p. 260.

Dr. HATFIELD, in replying, said that he first of all desired to record his appreciation of the excellent discussion which the paper had received. Perhaps the most valuable feature was the genuine openness of mind of all the contributors which made the interchange of ideas both pleasant and profitable. He felt amply repaid for any small effort of his in preparing the paper. His object had been to place before aero engineers reliable data concerning steels and their application, with the serious purpose of assisting to minimise or eliminate difficulties and troubles arising during manufacture and in service. Incidentally to provoke discussion upon various matters which, in his opinion, called for earnest attention. In the latter direction he felt that he had been successful. Indeed, the discussion had been such a full one that, without preparing another paper in reply, it was almost impossible to deal with all the points raised.

He had been pleased to see Professor Carpenter present, particularly as it was largely due to the work of the Professor and Mr. Keeling that they were indebted for the data upon which the present iron-carbide diagram was built. He hoped his paper had resulted in still further indicating the close relationship which must needs exist between the pure and applied science.

With regard to the elastic limit determinations, he was interested in the remarks made by Professor Carpenter, Colonel Bagnall-Wilde, Dr. Rosenhain, Dr. Thompson, and others. His (Dr. Hatfield's) view was that the engineer might usefully familiarise himself with the general values of the limit of proportionality for the diverse conditions of different steels, but as the readings demanded thoroughly skilled observers and considerable time, these considerations militated against the figure becoming other than a factor in experimental work. He was pleased that Professor Carpenter had emphasised the point raised in the paper with regard to the special opportunity existing in small aeroplane parts for careful heat-treatment. In the paper he had discussed this matter at some length, and could only reiterate that he felt sure that any extra time and attention given to the hardening and tempering shop would be amply repaid in the properties and reliability of the final parts.

Much had been said upon the impact test, and the statement in the paper to the effect that this form of test really merited the careful attention of unbiassed investigators had been amply confirmed. For instance, his friend Dr. Rosenhain

considered that he could discriminate by means of the microscope between high and low impact materials, whilst Mr. Dickenson apparently held that no such evidence could be obtained by the microscope. As a matter of fact, in his opinion both of them would probably be found to be partly correct. Generally, the changes in impact value were accompanied by changes in the values obtained in the tensile and other tests, and when that was so he would be prepared to admit that, generally, but not invariably, Dr. Rosenhain's contention was correct. There was, however, definite evidence that, with very little variation in tensile figures, differences are brought out by the impact test, which cannot—at any rate, under ordinary magnification—be explained by the micro-structure. It will thus be clear that statements apparently contradictory are equally true in a limited sense, and incidentally one might observe that this usually is so when the speakers, as in the present case, are men with an intimate knowledge of their subject. He noticed that Mr. Dickerson definitely stated that steel giving only 16 foot-lbs. on the Izod impact testing machine may be described as "brittle." He (Dr. Hatfield) would suggest that if that speaker surveyed steels in general in a broad manner, in conjunction with their impact values, and the work which they were called upon to do, he would find such a statement to be an unjustified one. Various steels largely used might well be cited which would not be considered "brittle" at such a figure. At any rate, their behaviour in service under stringent conditions certainly does not justify their being so described. The impact figure represents the number of foot-lbs. of energy required to smash the notched 10 mm. square test-bar, and it will be appreciated that if smashing under impact is to be taken as an indication of "brittleness," a figure which indicates "brittleness" in one steel must be a similar index of the same condition in another.

With regard to the round impact test-piece developed by Colonel Bagnall-Wilde and Captain Philpot, mentioned by several speakers, he (Dr. Hatfield) saw no reason why the round form should not replace the square one for general purposes, providing it is properly standardised.

Sir Charles Parsons had asked what impact value would be obtained from manganese steel in the toughened condition, and had suggested that it would probably be a good one. Sir Charles was perfectly right. The figures obtained from such material were very good.

Whilst speaking of impact testing and brittleness it might be useful to consider the statements made in the discussion by Mr. Fowler, Colonel Bagnall-Wilde, and Dr. Rosenhain to the effect that low impact values are almost always associated with failure. He would suggest that before you can determine the effect of one particular factor, that factor must be the only variable. For instance, we were told that crankshafts failed because of low impact values, but was it not relevant to ask whether such factors as faulty design, faulty workmanship, metallurgical and other defects, etc., had been eliminated in such cases? Incidentally, it might be asked whether numerous crankshafts which have had a successful life have been examined with the same detailed care as those which have failed from various causes. He (Dr. Hatfield) had frequently known the examination of good examples to modify definitely the opinions formed upon data derived from bad ones. Incidentally, when examining the impact value of a part which had failed, care should be taken that the impact piece was cut from a portion of the specimen sufficiently far removed from the fracture to truthfully represent the original condition of the material. Mr. Rolfe and Mr. Stubbs had also commented in an interesting manner upon the usefulness of the impact test, and he quite appreciated their point of view. In leaving this question of impact testing, it was, of course, clear that a mass of interesting and valuable data had been, and is being, collected which, when properly considered, would no doubt be quite useful. The dicussion had, however, brought out differences of opinion which must be reconciled. As regards the part played, or to be played, by this

form of test in specifications, he (Dr. Hatfield) had no desire to make any comment. He had dealt with the subject from the detached standpoint of the investigator and simply asked that the impact value should not attract such predominating attention as to result in the exclusion of a proper consideration of other important factors discussed in the paper.

He much appreciated Mr. Remington's remarks, and agreed with him that the engineer and metallurgist were now evidently collaborating in the manner which the close relationship of engineering and metallurgy demanded. Mr. Remington's own activities had had quite an influence upon this development. Particularly was it a pleasure to find an engineer like Mr. Berriman handling the metallurgy of steel with such sympathy and insight. With regard to the points raised by Mr. Remington, he thought the example which he gave of a crankshaft improved by putting a saw-cut instead of a radius at the change of section was a particularly interesting one. The explanation given, that the greater local deflection causes a redistribution of stress, was quite reasonable. There was, however, another possible reason which could be suggested for the increased strength, viz., that in the first sketch there are two corners directly opposite to each other on different sides of the web; in the improved design the corners are separated in a direction parallel to the web. The combined effect of the two corners of the first view would naturally tend to cause weakness. He was pleased Mr. Remington had requested him to specify what he meant by the word "shock" as applied to extreme cases of acceleration. He quite agreed that it was necessary to avoid confusion as to the meanings applied to this term. A live load he (Dr. Hatfield) took to be one which is applied to a particular member suddenly in such a way that the inertia of the moving parts carries the "straining" effect temporarily beyond that which would be produced if the same load were applied steadily. A "load supplied with shock" he took to be one which, besides being applied suddenly, comes into action with a certain amount of kinetic energy, which has to be absorbed or dissipated in some way. The "pressure of explosion" in its effect on the piston rod of an internal combustion engine approaches to the ideal of a "live load," whilst the impact of a valve on returning to its seat is an example of a "shock." Mr. Remington also asks for a statement as to the meaning of the word "soft" as applied to case-hardening steels. He (Dr. Hatfield) would suggest that the term "soft" used in the sense employed in the paper means capability of a large amount of plastic deformation.

With regard to Mr. Berriman's remarks, he was in general agreement and was pleased to see that that speaker, with considerable experience behind him, emphasised the necessity of the engineer seeing that the steel received suitable heat-treatment. As Mr. Berriman says, "their use in the raw state is both dangerous and waste of money." The author looks forward to the time when engineers generally will have realised that a little more intimate knowledge of the constitution and treatment of steel will have enabled them to understand its great possibilities.

He much appreciated Professor Dalby's contribution. It was naturally particularly useful to have his comments upon tensile testing, of which subject the Professor had of late made such a special study. He would like to commend Professor Dalby's method of taking stress-strain diagrams to engineers as one likely to be of the utmost service to them. As regards the semi-independence of elongation and reduction of area, he regretted that that portion of his paper had perhaps not received the amount of discussion which he had hoped.

With regard to Mr. Dickinson's remarks concerning the method of making crankshafts "in which the bloom is forged to crank shape and finished between dies," in which he (Mr. Dickinson) takes particular interest, it was of interest to have his assurance that "the considerable care required in such process" was given to that method of production.

As a result of Dr. Rosenhain's contribution, he felt that they were substantially in agreement, and he was particularly pleased to notice the emphasis which had been given to the troubles accruing in the welding operation. He was also pleased to note that Dr. Rosenhain shared his views with regard to the necessity of complete pyrometric control. Dr. Rosenhain opened by pointing out that there is now substantial agreement amongst metallurgists with regard to the main factors of steel metallurgy and that the engineer may now, with confidence, accept a large mass of facts which can, and must be, of inestimable service to him, and this general statement of Dr. Rosenhain's would, he felt sure, have the support of all serious metallurgists. The question of examining the manner in which the silicon occurred in the steel, *i.e.*, as to how much occurred as SiO_2 in the form of slag inclusions and how much as silicon in solution as FeSi in the steel, was an interesting one, and one which would admittedly be of immediate scientific interest, and probably, ultimately, of works interest.

With regard to Mr. Stubbs's remarks, he quite agreed that chrome-vanadium steel was a very useful one. With regard to air-hardening steel, obviously, owing to its particular properties, it would have to be studied very closely by the user, and Mr. Stubbs's comments *re* its manufacture into drop forgings were clearly to the point; it was necessary that the particular properties of this steel should be carefully studied and the design of the forging limited to the capabilities of the steel. He was glad to note Mr. Stubbs's comments *re* the value of stainless steel for valves, and could personally confirm the observations made. With regard to Mr. Fowler's query as to the properties of cold-worked stainless steel, particularly in connection with R.A.F. wires, he would say that they had investigations at present in hand covering the ground mentioned. He would take an early opportunity of communicating with Mr. Fowler direct in connection with this matter.

He was much interested in Dr. Thompson's contribution, which raised one or two particularly useful points. It was of interest to see Professor Arnold's alternating stress test discussed, and the date provided by Dr. Thompson might usefully be considered along with the comments found in the paper. He had followed with considerable pleasure Dr. Thompson's recent papers dealing with the effect of crystal size, etc., on the mechanical properties, and work in that direction, he felt sure, would result in a much more complete understanding of the properties of steels and other metals.

The comments made by Sir Charles Parsons, Mr. Fowler, Colonel Bagnall-Wilde, Professor Carpenter, and others, on the utility of aluminium and its alloys would be of more than general interest to the engineers engaged in aero work.

He quite agreed with Colonel Bagnall-Wilde's suggestion that a paper on "Steel Tubing" would be a valuable one and of immediate interest, and he (Dr. Hatfield) hoped that the Council of the Aëronautical Society would take the hint and prevail upon some expert in tubes to give them a paper.

In conclusion, he was afraid that he had not, in the space at his disposal, been able to adequately deal with all the points raised in the discussion, but he much appreciated the interest which had been taken in the paper.

At the termination of the evening General Ruck proposed a vote of thanks to Sir Charles Parsons for his presence in the chair, which was unanimously carried.

NINTH MEETING, 52nd SESSION.

An ordinary general meeting of the Society was held on Wednesday, May 2nd, at 8.0 o'clock, in the Theatre of the Royal Society of Arts, Adelphi, London. In the absence of Sir Walter Lawrence, K.C.I.E., who had been invited to preside, Major LYONS, F.R.S., President of the Royal Meteorological Society, took the chair.

Captain C. J. P. CAVE, R.E., gave a lecture on "Some Meteorological Conditions which Increase the Danger of Flying."

A discussion followed.

Sir NAPIER SHAW, F.R.S.: Captain Cave has put vividly before us the terrors of meteorology, but I think that I, personally, should take a different view. Airmen have a magnificent opportunity of telling us all about the weather. In the old days we had to take observations from the ground; now we have machines that will take our observers thousands of feet into the air.

In those days we saw birds flying about in the empyrean, but they told us nothing; and to-day we have big birds flying above our head, but they will tell us nothing. They have laughed at the cyclone, and they have derided squalls, and that is all they do do. Captain Cave showed us many things that we have found out from the ground. But to-day people can fly into a thunder cloud or into a cumulus cloud and tell us what it is made of. To me what matters is not so much what the meteorologist can tell the flyer as what the flyer can tell the meteorologist.

There is an old game called "Hunt the Thimble." The thimble was hidden and the victim was brought into the room where the people sitting about refused to give him any information. From various indications, a turning of the eye, or so forth, the thimble was ultimately discovered. Airmen are playing this game with meteorologists to-day. They sit about and refuse to speak. What I do desire to do is to urge upon those who can fly the great opportunity that they have of increasing our knowledge of the atmosphere. The phenomena of weather are immense and spread over a great surface. The effects are immense. The prosperity and happiness of practically every inhabitant of the globe depends to the last degree upon the answer to the one question, "What of the weather?" I can only plead that after the war, those airmen who have so gallantly shown their ability to ride the whirlwind in war will, when peace dawns again upon the world, help us forward to even greater conquests over the secrets of science.

Mr. BERTRAM COOPER: The principal question I wished to raise has just been raised, that is, the height to which it might be necessary to go to get over line-squalls. The matter is of great importance for the commercial future of flying. It has been suggested that a low level of cross-country flying for commercial purposes should be adopted, and when we reflect on the handicap that the necessity of being "high climbers" would be to commercial aircraft, owing to the falling off in lifting capacity at heights, we see that it would be very desirable to have commercial air routes at about 3,000ft. or so. But will the weather permit of this? Of the disturbances mentioned by Captain Cave "line-squalls" seem the most formidable opponents of a regular air service on particular routes, unless one can rely on always being able to get over them pretty easily. Within limits, the less speed and climb we have to give to our commercial aircraft, the more power we shall have for weight carrying, so that the matter is very important.

As regards the lack of interest shown in meteorology by flyers, it seems to me matters will be much better after the war. It is true we now have thousands of flyers, but they are flying under war conditions, and are in much greater danger from war perils than from weather; hence the latter takes a back seat. But after the war I certainly look forward to the establishment of an immense meteorological service in the air. It will be essential, in fact, for commercial purposes.

Captain FRESHVILLE inquired whether there was any limit to the height to which it was possible to fly.

The CHAIRMAN said that we had had a very interesting lecture and quite agreed that fuller knowledge was required. We ought to have a fuller organisation for transmitting information.

He desired to express, on behalf of the meeting, their great indebtedness to Captain Cave for his excellent lecture.

Captain CAVE replied very briefly.

Major Lyons was thanked for presiding and the meeting terminated.

TENTH MEETING, 52nd SESSION.

THE TENTH MEETING of the FIFTY-SECOND SESSION of the Society was held on May 16th, in the Theatre of the Royal Society of Arts, at 8.0 p.m. In consequence of the indisposition of Dr. Dugald Clerk, F.R.S., who had been invited to preside, Mr. HENRY FOWLER, Superintendent of the Royal Aircraft Factory, took the chair.

Mr. LOUIS COATALEN read his paper on "Aero Engines."

Mr. A. J. ROWLEDGE: I very much appreciate the honour of being asked to open the discussion on Mr. Coatalen's paper. I had no idea until this moment that I should be asked to do this, nor even had I any intention of taking part in the discussion. In the main, I do not find anything in the paper with which I disagree, and can only say that I should like to congratulate Mr. Coatalen on the very complete paper that he has presented to the Society.

Major GREEN stated that after listening to the valuable paper by Mr. Coatalen, one realised how much we owed to the war in hastening the progress of aero engines. At the same time no doubt Mr. Coatalen's paper would have been even more interesting had he been able to give some figures on stresses, bearing loads, and so forth, which would have been of valuable assistance to all aero engine designers.

When considering the various types of aero engines that were in use, one could not help being surprised by the number of totally distinct types for all of which good use was being found, and the remarkable way in which one could arrive at useful engines by different methods. As an instance, the lightest engine in the past was the Gnome, or rotary type, and the heaviest engine had been the 6-cylinder vertical water cooled type developed from motor car engines. By sticking to the development of one type it had been found possible to reduce the weight of the 6-cylinder motor until it was beginning to approach very closely to the rotary type.

In regard to the carrying of lubricating oil outside the base chamber, Major Green did not agree entirely that the reason for doing this was for cooling purposes. It would be possible to cool the oil carried in the base chamber if suitable cooling ribs were cast on, and arrangements made for a draught over the base chamber. Many engines had run successfully in this way. The reason that the oil was carried outside the base chamber was due primarily to the fact that the modern aeroplane was expected to dive vertically, and in consequence if oil were carried in the base it is nearly impossible to prevent the front cylinders getting over oiled.

Major Green stated that he did not understand why it was obvious the best position of the sparking plug was in the centre of the head. He would be glad if Mr. Coatalen would give some information on this subject.

With regard to the head resistance question there seems to be a good deal of misunderstanding on this subject. It was generally assumed that a 6-cylinder engine was of very much lower head resistance than the rotary type of engine. In practice this was not necessarily so, as if the engine were put in front of the body, so long as it was not of greater diameter than the body the resistance of

the aeroplane would not be increased. It was necessary also to carry a radiator with water cooled engines, and it was primarily the resistance due to cooling that made up the resistance of the engine. In the rotary engine, which cools itself by revolving on its own axis, the power necessary in cooling was already to a large extent subtracted when giving the stated horse-power to the engine, consequently with two motors of the same nominal power it was very often possible to make a lower resistance body with the rotary than with the 6-cylinder engine.

Major Green then congratulated Mr. Coatalen on the valuable work he had done in designing so many experimental types of engines, and he also congratulated Mr. Coatalen's Directors who were broad minded enough to see him through work that was of first importance to the nation.

Mr. G. C. Loening: Mr. Coatalen appears to me to have touched on a most important point in emphasising the distinction between automobile and aviation motors, as it is clear that the backwardness in motors in America is largely due to a lack of appreciation of this point, and his lecture, based on his expert experience, will help to alter this. I should be glad to know whether there are not many automobile and motor features still to be found in aviation motors which are illogical and undesirable. For example, the usual arrangement of crank case, etc., so desirable in an automobile, made mounting in an aeroplane clumsy and difficult to take care of without a great deal of extra bracing and, therefore, weight, in the engine bed construction. Add to this the fact that the thrust, particularly on direct drive motors, is too low. I am also anxious to know whether it would not perhaps be much better to boldly design a motor to be mounted on the longerous of the fuselage in a more direct manner, and even turn the engine upside down and mount it on its cylinders—a feature which the dry sump high pressure oiling systems would now more readily permit of.

I take exception to Major Green's statement relative to radiators, and would point out that experiments conducted in the United States had shown that the usual front radiator is quite unsatisfactory in comparison with a radiator built with the wing above the motor with the header forming the leading edge of the wing. The latter arrangement on test has proved capable of cooling the motor with less radiation and less water, thereby saving weight, and by permitting of a perfect streamlining of the nose of the fuselage reduced the resistance of the body more than enough to make up for the small radiator out in the open, which on a high-speed machine became very small indeed, due to the higher air speed than the cells. In addition, the air flow from the propeller is smoother, thus reducing vibration, and also not only is there a greater efficiency, but the cooling is more uniform, due to better circulation and the elimination of air pockets. On a seaplane this deposition is particularly desirable in that the motor cooled perfectly, even when the machine was slowly taxi-ing.

In closing my remarks, may I again express my sense of the high value of the lecture. I should be most grateful if I could be obliged with several copies immediately to forward to the United States, in the hope that our engineers may profit by the lecturer's remarks.

Lieut.-Colonel Mervyn O'Gorman said he could only approve and not criticise Mr. Coatalen's excellent lecture. It was of special value to have from a maker of cars, and indeed from an eminent maker of racing cars, the support for what was a thesis he himself had upheld—that the new engine was *not* a car engine "gratté," as the French say. It had its own dominant problems and difficulties, and it had not even been evolved from the car engine. The lecturer gave 10 points of contrast. He would wish to add four more to these. One feature of the most vital importance frequently overlooked by inventors putting forward new aero engines was that the aeroplane engine must with unfailing certainty *restart after being cut off*. One well-known engine he thought the lightest engine ever

built had failed to enter the production stage, largely because it had in its early tests been proved to fail to restart. The two-stroke class of engines also, which offered, from a weight-saving point of view, special attractions, must, before it can be acceptable to pilots, be demonstrated to be at least as certain to restart after a long descent with engine cut off as is the ordinary four-stroke engine, and it is known that carburation is one of the problems with two-stroke engines. This requirement would be an eleventh point of contrast with the land engine.

A twelfth point is that the aero engine must be capable of accepting very steep angles of slope without either suffering from the oil running all to one end or otherwise failing or the carburettor betraying its trust. This demand was the real cause, in his opinion, of the use of the "dry sump" arrangement in aero engines, though he had learnt this evening that Mr. Coatalen had adopted the dry sump for another set of reasons. So critical had the question of draining all oil on protracted steep dives become that the trend of practice was to instal not one, but *two* oil pumps, one at each end of the sump, in addition to the extra pump alluded to by the lecturer. The one dealt with the oil on a steep climb, the other on a steep dive.

A thirteenth difference was the important disposition of the engine to bear the airscrew—not only its weight and its thrust, but also the gyrostatic couple and occasionally vibration caused by turning.

A fourteenth particularity was the necessity for protection from freezing while in the air at the really cold altitudes, involving control of the water circulation and variable shutters over the radiator. The question of excess cooling would be found to affect the oil cooler to which the lecturer alluded.

Lieut.-Colonel O'Gorman considered that aero engine design would react on car engines in countless ways. His own car had an engine of 920lbs. weight, rated at 25 h.p. and giving 40 h.p. For this weight he could now have, if he liked, about 220 h.p. in his car—or, better still, he could get the 40 h.p., which he found ample for a mere fraction of the old weight, with a design of engine which could work at full load for long hours in a way a car engine was never called on to do, save in racing, and with a great saving in the main cost of motoring, viz., tyres, the wear of which was dominated by the gross weight borne by them. The lecturer had rightly pointed out that in his car experience the non-technical demands of the buying public had exercised a considerable and on occasion detrimental influence on the design. He endorsed this view, and expanded it by saying that the value to the producer of a technical class of supply of having a technically critical and well-informed purchaser was very great. It was, of course, a nuisance to be unable to part with some part of his production which, in the course of evolution, was less good than his later efforts. The technically critical purchaser would not be lured into accepting anything less than the best by the simple expedient of painting the car body a brighter and more shiny colour —and similarly therefore also for aero engines purchased by test. The effect was to produce a far acuter pressure in the direction of progress, and to this extent therefore intelligent purchasing had some small share of the credit in the advances made.

The lecturer himself and his firm were distinguished by the extraordinary number of designs which they had been able to produce and try out in a given time, and he congratulated Mr. Coatalen on the way he had been able to *ginger up* his men by the initiative and drive of his own personality, and so to take a larger number of steps up the rungs of the ladder of progress than many others had been able to do.

He thought that the lecturer had overrated one objection to overall length of engines in suggesting that this was likely to affect appreciably the controllability

of the aeroplane. The introduction of very large horse-powers was in itself having far more important disturbing effects.

He had observed an aeroplane abroad in which were incorporated three engines, of which two engines were of eight cylinders in a line—an arrangement of maximum length and a most unpromising arrangement both from the point of view of engine balance, cost and weight, and springiness of crankshaft and weight of engine generally per horse-power. Yet in which, by actual experience, he had found that the vibration trouble was not important, and by measurement that the weight per horse-power was within the accepted limits of useful engine design of to-day. The long form happened to lend itself usefully to the particular design, and gave what was for a very large aeroplane (carrying 6,000lbs. of useful load) a very handy machine requiring small efforts to loop it and to control it. He thought that the only serious advantage of a short engine was that it saved weight per horse-power and did not require such expensive tools to prepare crankshaft forgings.

He wished to draw attention to one peculiarity in the relation which exists between aeroplane builders and engine builders. A new type of engine takes from the day that the design is started till it is produced in quantity, an interval of time which is at least 100 per cent. longer than the interval for the evolution of an aeroplane in quantity. The result of that relation is distinctly unfortunate both for the progress of the engine and the aeroplane, and it will remain so until such time as the two are designed and produced at leisure by one and the same designing office. That refers the matter to some period after the war.

Lieut.-Colonel O'Gorman pointed out that what occurs is somewhat as follows: The aeroplane designer in war time prefers to work out his aeroplane to make use of an engine which exists and is reliable and is likely to do him some credit at once. He is loth to study in all elaboration and to build an aeroplane in anticipation of some projected engine not yet tried out proving to be a success. He knows that there are so many slips twixt cup and lip in experimental engine production. That is to say, the tendency is for the aeroplane designer to concentrate his efforts on some engine the thinking out of which is about a year old. This may be inevitable, but it is rather hard on the engine designer, who is thereby divorced from actuality in his contact with aeroplane designers. The only manufacturing advantage he derives from this is that instead of being called on to build an engine which shall fit such or such an aeroplane for the best aerodynamic result, the boot is on the other foot, and it is the aeroplane man who has to take the engine as he finds it and elaborate his structure so as to carry it and "fair shape" it, and harmonise it with the other requirements imposed on him. From this it results that some engines are in fact heavier in use than they appear to be on the weighbridge, because they impose the necessity for an amount of structural material which other engines avoid.

On the subject of the rating of aero engines, he regretted that Mr. Coatalen had raised the point. In his own view, any engine which is chiefly used at full load and on which the purchaser inevitably conducted a b.h.p. and fuel consumption test required no "rating" formula whatever. Given equal reliableness, the basis of comparison between engines is not the horse-power per volume swept or per bore by stroke by revolutions, or any other of the abominations which were called in to enable a motor-car purchaser to know what size of engine he was purchasing without making a brake test. The basis of comparison was got by measurement of the value received. If a purchaser wanted very long hours of performance economy of fuel consumption cut an important figure, and he would carefully measure that and include it appropriately in the total weight. If the engine purchaser did not want the fuel load to be a particularly predominant matter, he need not worry about it so closely as he did about the b.h.p. per weight of the engine.

As regards the position of radiator so clearly preferred by Mr. Loening, he certainly did not, so far as experience had gone over here, find himself inclined to agree that the nett best result was to be got by putting the radiator away from the engine up in the centre section of the wing structure. To cool a given engine a certain amount of h.p. would have to be expended in establishing contact between the surfaces to be cooled and the ambient air, and this h.p. was not going to be diminished by putting the radiator in any particular position. The maximum economy to be gained was a measure of the maximum possible value of the procedure, and this maximum economy to be gained could be perfectly well known in advance of building the aeroplane by a simple wind tunnel experiment or by a pressure plotting on the full scale afterwards. The scheme consisted in utilising for cooling the head resistance of some known width of wing which the wing section must offer in any case as *part* of the head resistance of the radiator. This was the utmost advantage obtainable, and it was obviously small. The disadvantages were of a different order. A really light-weight radiator is a delicate thing, and to stretch it in a long narrow form in the nosing of a wing section was to expose it to greater chances of being damaged and shot than occurred when it was made as snug as possible. The pipes of communication from engine to wing nosing were long and delicate, and their joints exposed to all deformations of the aeroplane. The pilots were liable to bruise them; indeed it was possible to generalise and say that all lengthy pipes of thin light section about an aeroplane were to be shunned. Witness the far simpler proposition, the petrol pipe, and the trouble it has given for years. A radiator in such a position would be awkward to shroud, partially or fully, at will so as to prevent freezing when gliding down from great heights; it would be awkward to fill when in its shed. It would interfere, or might probably interfere with what is a very important gun position for firing over the wing and over the airscrew. No doubt Mr. Loening would have facts and measurements, but in ignorance of these he was not predisposed to the scheme.

Mr. HANDLEY PAGE: Mr. Loening has referred in the discussion to the subject of radiators and their position on the machine. From resistance tests in the wind tunnel it would appear that the resistance of our engine nacelles used on our large type of twin tractor biplanes is increased by approximately 25 per cent. when the radiators are placed at the side of the nacelle instead of in front of the engine immediately behind the propeller. It may be that if the radiators are placed completely away from the engine nacelle in the top plane, as suggested by Mr. Loening, that the resistance increase is not so great and that the increased cooling effect there may permit of the use of a smaller radiator. I should very much like to know whether Mr. Loening has any comparative figures on this point.

When the radiator is fitted in front of the engine it becomes a matter of extreme difficulty to get the necessary radiating surface without a large frontal area if the distance between the back of the propeller boss and the front of the engine cylinders is small. I would ask Mr. Coatalen whether it is not possible to increase this distance and, at the same time, keep the diameter of the reduction gearbox small so that there is as little as possible frontal area lost owing to engine obstruction of the air flow.

Lieutenant FILL, speaking on the three years' progress which Mr. Coatalen referred to in his paper, did not think the progress was anything like what it should be for the amount of experimental work carried out; in fact he thought there was too much experiment and not enough results. He admitted the h.p. had been increased wonderfully and the consumption decreased a little, but the reliable running of the engine had not been improved very much.

The bench test was not a proper test for an aero engine, and it was never tested the same as when running in the air. The engines should be tested by the

makers by running them for hours in the air, and all defects rectified before leaving the works.

Lieutenant Fill agreed with the lecturer as regards increasing the flow of the water, as most of the radiators fitted were too small.

When the British manufacturer could turn out an engine which would run regularly for six hours in the air, he would admit wonderful progress had been made.

Mr. BERTRAM COOPER: I don't know whether I am in order in commenting on a remark in the discussion, but I should like to take up the point just made about the noise of the airscrew being due to the irregularity of torque of the motor. It may be that, as the previous speaker pointed out, the note given out by the airscrew coincided in period with the explosion period of a certain six-cylinder engine, but that can only be regarded as a coincidence. I believe the R.A.F. tested airscrews run by an electric motor, with the result that the noise was not diminished, and it has been clearly demonstrated that the noise is due to the airflow round the blade being at such a high speed that sound waves are set up. Anyone who has been present when a large wind tunnel (such as the 7ft. one at the N.P.L.) is running full speed knows that the airscrew roar is simply appalling. As these airscrews are driven by electric motors, of which the torque is practically non-fluctuating, the noise cannot be due to the cause suggested by the speaker. I have wished to raise the matter because the speaker mentioned the fallacies of inventors, and I should not like any inventors to be misled by such a theory as has just been put forward.

Lieutenant ARCHIBALD LOW, R.N.V.R., said that for some two and a half years he had been watching assiduously the combination of engines and aeroplanes under something approximating to war conditions. He pointed out in reference to one of Mr. Coatalen's closing remarks that only at the beginning and end of a flight is an engine operating at sea level, and that tests on dry land are not by any means necessarily at sea level. He inferred that, consequently, h.p.'s procured under such varying conditions are not necessarily accurate. He said that he has never yet seen engine test-sheets marked with barometric, thermometric, and atmospheric-density readings prevailing at the time of the test.

Constructors might, perhaps, ask users what such things mattered to them if results were attained, and if they were given the necessary power for weight. He pointed out that in calculating the ceiling of a machine one has to take in the percentage of mechanical losses, and that in selling a motor to an engine user a designer may be asked what are his mechanical losses. He pointed out that in a rotary engine the purely mechanical losses are less than in a stationary cylinder engine, so that other things being equal, the ceiling for a rotary will be higher than a stationary, given the same power at sea level. Weight per h.p. operates as the square and not directly, and it is a mistake to think that mere h.p. irrespective of weight is not important.

THE REPLY.

Mr. LOUIS COATALEN: It appears to me that the matter has been touched on at such length that the briefest of replies is called for, particularly as Mr. Handley Page has answered the material point brought forward by Mr. C. E. Loening. He is also in part answered by Lieut.-Colonel Mervyn O'Gorman. Mr. Handley Page's own question about engine design can be described as essentially a practical point and one seemingly capable of solution. I agree with Mr. Loening to the extent that, undoubtedly, aircraft construction is so young a branch of engineering

that, in a sense, we have scarcely begun to tackle the problem of engine installation. From the outset that has been a considerably difficult one. Indeed, it is surprising that in practice schemes have worked out as satisfactorily as on the average to date. Undoubtedly, great and very rapid progress will be achieved under this head.

Touching Lieutenant Fill's remark that he does not think the progress recorded has been equal to the amount of experimental effort expended, I am qualified to speak from personal experience only. As far as it goes, assuredly it is matter for astonishment that we have been able to make such relatively great advance for comparatively such slight experimental cost. I recognise, however, that there can be no general rule about these matters. What the cost of the entire experimental effort of the aircraft engine industry in these islands, or in the whole of Europe, is to the rate of progress achieved, I cannot say. As to the improvement towards reliable running, I submit that there has been great advance, particularly when it is held in mind that before the outbreak of war engines of far less efficiency than those with which we are dealing to-day were handled only by groups of picked men. Usually they built the machines and understood the why and the wherefore of every point of their construction; whereas nowadays we are standardising engines of much higher output and are issuing them wholesale, many of them to the tender mercies of those who are not expert concerning them. By that I mean that the average mechanic working on them is quite good enough as regards maintaining the car type of engine—a vastly different undertaking. The British Air Services have been greatly handicapped in having had to make the majority of recruits understand the utterly new class of engineering construction they are called on to deal with. All factors considered, I believe they are making remarkable progress under this head. In connection with the reliability of engines on aircraft, I would also take the opportunity to draw attention to the fact that when constructors of certain types of aircraft consider engines available, in selecting one of so many horse-power generally they are not able to design a machine to fly with, say, three-quarters or half that horse-power. Instead, they must so scheme that to get the given aircraft launched in flight will need the full horse-power the given engine is capable of developing. At present they cannot help themselves, save in the case of fighting machines that must have a good reserve of power. I have drawn attention already to the fact that the extraordinary reliability of motor car engines nowadays is due to their being rarely required to develop their maximum horse-power for more than brief spells. In the beginnings of that movement, however, they had to run at full power for long periods and were very unreliable in consequence. An aircraft engine is capable of better things; but it will be allowed that there is a happy mean between that and developing maximum horse-power all the time. That mean lies in the direction of developing normal horse-power all the time, there being always a goodly margin between normal and maximum horse-power. Maximum power should be held in reserve for use for brief spells only in emergency or other exceptional circumstances. Further, there is the magneto problem over which the aircraft engineer can exercise no control.

In regard to Lieutenant Fill's remarks touching engine tests approximating more to flying conditions, I argued in favour of this in aircraft engine competitions proposed even before the war. As far as individual experience goes, I find that some of the tests we have to do with aircraft engines in the running sheds, for example, represent much more severe trials of durability than any that obtain in the air, where the engine, on a more plangent bed, is not subjected to anything like the fatigue. As for engines running reliably for six hours at a spell in the air, provided they are properly maintained this is but child's play compared with the performances required under test of aircraft engines of to-day. Our engines do much more than six hour flights.

Lastly, Lieut.-Colonel Mervyn O'Gorman makes some original additional points of which most will approve. I thought I had made it abundantly plain in the body of the lecture that it was only for certain sorts of aircraft that overall length was an objection, but that for others relatively great overall length to achieve minimum head resistance was an advantage. Development in regard to engine length, to which he refers, can be endorsed from my own enterprise.